OBSERVATIONS

ON THE

NATURAL HISTORY

OF

BEES.

BY

FRANCIS HUBER.

A NEW EDITION,

WITH A MEMOIR OF THE AUTHOR, PRACTICAL APPENDIX,

AND ANALYTICAL INDEX.

ILLUSTRATED WITH ENGRAVINGS.

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ADVERTISEMENT

So far as the Publisher is aware, nearly twenty years have elapsed since the last English edition of *Huber's Observations on Bees* was given to the Public. Under these circumstances, the acknowledged importance of Huber's discoveries renders any apology unnecessary for the appearance of this Edition, which it is hoped will be found more accurate and complete than any other previously published. The text has been carefully revised and rendered more agreeable to the English idiom than that of former translations. A copious Appendix has been added, containing information on several branches of Practical Management, hitherto regarded by many Bee-Masters as a *desideratum* to the original Observations. The Publisher has also prefixed an Abridgment of De Candolle's Memoir of the Author, considering such as necessary to the perfection of the Work—at the same time as a merited tribute to the indomitable ardour and genius of Huber. An Analytical Index has likewise been compiled, which will be found not only to facilitate reference, but to afford a very satisfactory synopsis of the volume.

TRANSLATOR'S PREFACE.

The facts contained in this volume are deeply interesting to the naturalist. They not only elucidate the history of those industrious creatures whose properties are the peculiar subject of investigation, but they present some new and singular features in animal physiology, hitherto unknown, and even unsuspected.

In all countries, and in every age, the labour of bees has proved a fertile source of admiration, and mankind have endeavoured unremittingly to convert it to gratification or emolument. What can be more wonderful, indeed, than to witness an insect of such apparent insignificance rendering each different flower tributary to itself or the necessities of its young, and fabricating structures which no human art can approach or imitate! But it is from the impulse of its propensities, and from the united efforts of myriads, that we are enabled to gain those valuable products, which otherwise would be utterly unattainable.

Curiosity and avidity being equally awakened by the industry of these diminutive beings, innumerable theories, observations, and experiments have followed regarding them, and uncommon patience has attended the inquiry. Yet the real discoveries, compared with the multitude of investigators, have been surprisingly few. Vague speculations have been substituted for rational researches into the nature of bees, and superficial inspection deemed satisfactory analysis of their works. Hence the properties actually ascertained are so interwoven with errors, that no subject has been the parent of greater absurdities. Unfortunately, also, some of those treatises, the most accessible, and, at the present day, the most popular, only contribute to their wider dispersion, and enhance the difficulties of the philosophic naturalist attempting their eradication. A considerable portion of the subsequent work is devoted to this purpose. It belongs to the reader to judge of the deductions whereby the author concludes that he has established facts from experiment.

Perhaps no treatise of equal compass, perhaps no treatise whatever contains as many novelties in the history of bees. Their nature, organization, senses, instinct, and mode of perpetuation, are all illustrated. The origin of wax, the faculty of obtaining it from honey or sugar, its application to use in the structure of cells, and the formation of combs, are perspicuously discussed, while several points are established which had been previously the alternate theme of conjecture and controversy. But the general approbation given here, as well as on the Continent, to a modest and unobtrusive work, wherein both instruction and amusement are combined, constitutes the best testimony of its merits. Thus, to use the words of Sue, a foreign author, "the observations are so consistent, and the deductions so conclusive, that, when occupied with this treatise, we feel as if we had assisted the author in each experiment, and pursued it with corresponding zeal and interest. Let us invite the admirers of Nature to its perusal; few are of such quality, or so faithfully describe the properties and habits of bees."

It is a circumstance too remarkable to be overlooked, that the author's defective sight induced him to employ an assistant in conducting his experiments. Some of these have been since confirmed by various observers, and among others by his son Peter Huber, whose elegant taste for natural history is testified in his *Researches concerning the Habits of Ants.* The experiments themselves seem to have been so judiciously adapted to the object in view, and the conclusions so strictly logical, as evidently to preclude important errors. It is not uncommon, indeed, to hear authority challenged. Even persons the least qualified venture to oppose their opinions to the truly learned, with a specious air of confidence, very apt to delude the unthinking part of mankind. But those whose minds are the most copiously stored, are not invariably the most urgent to disclose the full scope of their information. Therefore, although certain facts be not specified, it is far from a necessary consequence that they have been unseen; nor although we fail to procure the same result under what we deem the same circumstances, are we hastily to denounce the accuracy of our precursors.

After the talents of the author's philosophic assistant, Francis Burnens, had been assiduously devoted to his service for a number of successive years, he was called on to reside in a different quarter, where he now discharges some respectable public functions; and it appears that of late he had not forsaken their common pursuits.

A long interval elapsed between the publication of the different memoirs, forming the first and second part of this volume. Here the whole are presented somewhat in an abbreviated form, particularly the portion relative to anatomical details, the theory of respiration, and the architecture of bees. All the substance, however, is preserved; and, amidst the perplexity and intricacy in which it is partly involved, the narrative is rendered as explicit and concise as the subject admits. Nevertheless, a few passages have proved of doubtful interpretation, and a few unintelligible. Naturalists are frequently betrayed into an unprofitable prolixity, lessening the interest of their works, and even clouding them with obscurity.

The cultivation of bees forms a branch of rural economy, which may be carried to very great extent. Its utility is obvious; and where so much benefit might accrue, especially to the more dependent classes of the community, it is to be regretted that it does not receive greater attention. In general the cultivator will find his cares sufficiently rewarded.

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PART II.

APPENDIX.

Page 163. The distance specified for the excursions of bees seems to be corroborated by a communication to M. Lombard from the author, in April 1810.

MEMOIR OF HUBER.

FRANCIS HUBER was born at Geneva, on the 2d of July 1750, of an honourable family, in which vivacity of mind and imagination seemed hereditary. His father, John Huber, had the reputation of being one of the most witty men of his day; a trait which was frequently noticed by Voltaire, who valued him for the originality of his conversation. He was an agreeable musician, and made verses which were boasted of even in the saloon at Ferney. He was distinguished for lively and piquant repartee; he painted with mnch facility and talent; he excelled so much in the cutting out of landscapes, that he seems to have been a creator of this art. His sculpture was better than that which those who are simply amateurs are able to execute; and to this diversity of talent, he joined the taste and the art of observing the manners of the animal creation.

John Huber transmitted almost all his tastes to his son. The latter attended from his childhood the public lectures at the college, and, under the guidance of good masters, he acquired a predilection for literature, which the conversation of his father served to develope. He owed to the same paternal inspiration his taste for natural history, and he derived his fondness for science from the lessons of De Saussure, and from manipulations in the laboratory of one of his relatives, who ruined himself in searching for the philosopher's stone. His precocity of talent was manifest in his attention to nature, at an age when others are scarcely aware of its existence, and in the evidence of deep feeling, at an age when others hardly betray emotions. It seemed that, destined to a submission to the most cruel of privations, he made, as it were instinctively, a provision of recollections and feelings for the remainder of his days. At the age of fifteen, his general health and his sight began to be impaired. The ardour with which he pursued his labours and his pleasures, the earnestness with which he devoted his days to study, and his nights to reading of romances by the feeble light of a lamp, and for which, when deprived of its use, he sometimes substituted the light of the moon, were, it is said, the causes which threatened at once the loss of health and of sight. His father took him to Paris, to consult Tronchin, on account of his health, and Venzel on the condition of his eyes.

With a view to his general health, Tronchin sent him to a village (Stain) in the neighbourhood of Paris, in order that he might be free from all disturbing occupations. There he practised the life of a simple peasant, followed the plough, and diverted himself with all the rural concerns. This regimen was completely successful, and Huber retained, from this country residence, not only confirmed health, but a tender recollection and decided taste for a rural life.

The oculist Venzel considered the state of his eyes as incurable, and he did not think it justifiable to hazard an operation for cataract, then less understood than at present, and announced to young Huber the probability of an approaching and entire blindness. His eyes, however, notwithstanding their weakness, had, before his departure, and after his return, met those of Maria Aimée Lullin, a daughter of one of the syndics of the Swiss Republic. They had been companions at the lessons of the dancing master, and such a mutual love was cherished as the age of seventeen is apt to produce. The constantly increasing probability, however, of the blindness of Huber, decided M. Lullin to refuse his consent to the union; but as the misfortune of her friend and chosen companion became more certain, the more did Maria regard herself as pledged never to abandon him. She had become attached to him at first through love, then through generosity and a sort of heroism; and she resolved to wait until she had attained the lawful age to decide for herself (the age of twenty-five), and then to unite herself with Huber. The latter perceiving the risk which his infirmity would probably occasion to his hopes, endeavoured to dissimulate. As long as he could discern some light, he acted and spoke as if he could see, and often beguiled his own misfortune by such a conference. The seven years thus spent made such an impression on him, that during the rest of his life, even when his blindness had been overcome with such surprising ability as to furnish one of his claims to celebrity, he was still fond of dissembling: he would boast of the beauty of a landscape, which he knew of only by hearsay or by simple recollection,—the elegance of a dress,—or the fair complexion of a female whose voice pleased him; and, in his conversations, in his letters, and even in his books, he would say, *I have seen, I have seen with my own eyes.*

These expressions, which deceived neither himself nor any one else, were like so many recollections of that fatal period of his life when he was daily sensible of the thickening of the veil which was constantly spread between him and the material world, and increased his fear not only of becoming entirely blind, but of being deserted by the object of his love. But it was not so; Miss Lullin resisted every persuasion—every persecution even—by which her father endeavoured to divert her from her resolution; and, as soon as she had attained her majority, she presented herself at the altar, conducted by her maternal uncle, M. Rilliet Fatio, and leading, if we may so term it, herself the spouse who in his happy and brilliant days had been her choice, and to whose saddened fall she was now determined to devote her life.

Madame Huber proved, by her constancy, that she was worthy of the energy which she had manifested. During the forty years of their union, she never ceased to bestow upon her blind husband the kindest attention: she was his reader, his secretary, his observer, and she removed, as far as possible, all those embarrassments which would naturally arise from his infirmity. Her husband, in alluding to her small stature, would say of her, *mens magna in corpore parvo*. *As long as she lived*, said he also, *I was not sensible of the misfortune of being blind.*

We have seen the blind shine as poets, and distinguish themselves as philosophers and calculators; but it was reserved for Huber to give a lustre to his class in the sciences of observation, and on objects so minute that the most clear-sighted observer can scarcely observe them. The reading of the works of Reaumur and Bonnet, and the conversation of the latter, directed his curiosity to the history of bees. His habitual residence in the country inspired him with the desire, first of verifying some facts, then of filling some blanks in their history; but this kind of observation required not only the use of such an instrument as the optician must furnish, but an intelligent assistant, who alone could adjust it to its use. He had then a servant named Francis Burnens, remarkable for his sagacity and for the devotion he bore for his master. Huber practised him in the art of observation, directed him to his researches by questions adroitly combined, and aided by the recollections of his youth, and by the testimonials of his wife and friends, he rectified the assertions of his assistant, and became enabled to form in his own mind a true and perfect image of the minutest facts. "I am much more certain," said he one day to me, smiling, "of what I taste than you are, for you publish what your own eyes only have seen, while I take the mean among many witnesses." This is, doubtless, very plausible reasoning, but it will hardly render any one mistrustful of his own eyes!

The publication of his observations took place in 1792, in the form of letters to Ch. Bonnet, and under the title of "*Nouvelles Observations sur les Abeilles**." This work made a strong impression upon many naturalists, not only from the novelty of the facts, but from their rigorous exactness, and the singular diffi-

* One vol. 8vo, Geneva. Another edition was printed in Paris in 1796, in one volume 12mo; in which a short practical treatise on the management of bees was anonymously subjoined to the works of Huber.

culty against which the author had to struggle with so much ability. Most of the academies of Europe (and especially the Academy of Sciences of Paris) admitted Huber, from time to time, among their associates.

The activity of his researches was relaxed neither by this early research, which might have satisfied his self-love, nor by the embarrassments which he suffered in consequence of the Revolution, nor even by a separation from his faithful Burnens. Another assistant of course became necessary. His first substitute was his wife,—then his son, Pierre Huber, who began from that time to acquire a just celebrity in the history of the economy of ants, and various other insects, commenced his apprenticeship as an observer, in assisting his father. It was principally by his assistance that he made new and laborious researches relative to his favourite insects. They form the second volume of the second edition of his work published in 1814, which was edited in part by his son.

The origin of the wax was at that time a point in the history of bees much disputed by naturalists. By some it was asserted, though without sufficient proof, that it was fabricated by the bee from the honey. Huber, who had already happily cleared up the origin of the *propolis*, confirmed this opinion, with respect to the wax, by numerous observations; and showed very particularly, with the aid of Burnens, how it escaped in a laminated form from between the rings of the abdomen.* He instituted laborious researches to

* The work of Huber on this subject appeared in the Bibliotheque Britannique, under the title of "Premiere Memoire sur l'Origine de la Cire," t. xxv. p. 59; but they

discover how the bees prepare it for their edifices; he followed step by step the whole construction of those wonderful hives, which seem, by their perfection, to resolve the most delicate problem of geometry; he assigned to each class of bees the part it takes in this construction, and traced their labours from the rudiments of the first cell to the completed perfection of the comb. He made known the ravages which the *Sphinx atropos* produces in the hives into which it insinuates itself *; he even endeavoured to unravel the history of the senses of the bees, and especially to examine the seat of the sense of smell, the existence of which is proved by the whole history of these insects, while the situation of the organ had never been determined with any certainty. Finally, he prosecuted a curious research into the respiration of bees. He proved by very many curious experiments that bees consume oxygen gas like other animals. But how can the air become renewed, and preserve its purity in a hive plastered with cement, and closed on all its sides, except at the narrow orifice which serves for a door? This problem demanded all the sagacity of our observer, and he at length ascertained that the bees, by a particular movement of their wings, agitated the air in such a way as to effect its renovation; and having assured himself by direct observation, he farther proved its correctness by means of artificial ventilation.

have been resumed and extended in the second edition of his Researches.

* This part of his researches had already appeared in the Bibliotheque Britannique, in 1840, t. xxvii. pp. 275 and 358, under the title of "Letter to M. Pictet."

This perseverance of a whole life, in a given object, is one of the characteristic traits of Huber, and probably one of the causes of his success. Naturalists are divided from taste, and often from position, into two series. The one love to embrace the *tout ensemble* of beings, to compare them with others, to seize the relations of their organization, and to deduce from them their classification and the general laws of nature. It is this class who have necessarily at their disposal, vast collections; and they mostly dwell in large cities. The others take pleasure in the profound study of a given subject, considering it under all its aspects, scrutinizing into its minute details, and patiently following it in all its peculiarities. The latter are generally sedentary and isolated observers, living remote from collections, and far from great cities.

Huber is evidently to be placed in the school of special observers: his situation and infirmity retained him in it, and he acquired therein an honourable rank, by the sagacity and precision of his researches; but it is plainly perceptible, in reading his works, that his brilliant imagination urged him toward the region of general ideas. Unprovided with terms of comparison, he sought them in that theory of final causes, which is gratifying to every expanded and religious mind, because it appears to furnish a reason for a multitude of facts, the employment of which, however, as is well known, is prone to lead the mind into error; but we must do him the justice to acknowledge, that the use he makes of them is always confined within the limits of philosophical doubt and observation.

His style is, in general, clear and elegant; always retaining the precision requisite to the didactic, it possesses the attraction which a poetic imagination can readily confer upon all subjects; but one thing which particularly distinguishes it, and which we should least expect, is, that he describes facts in a manner so picturesque, that in reading them, we fancy that we can see the very objects, which the author, alas, was never able to see! I venture also to add, that we find in his descriptions so many masterly touches, as to justify the conclusion, that if he had retained his sight he would have been like his father, his brother, and his son, a skilful painter.

His taste for the fine arts, unable to derive pleasure from forms, extended to sounds; he loved poetry, but he was more especially endowed with a strong inclination for music. His taste for it might be called innate, and it furnished him with a great source of recreation throughout his life. He had an agreeable voice, and was initiated in his childhood in the charms of Italian music.

The desire of maintaining his connection with absent friends, without having recourse to a secretary, suggested the idea of a sort of printing press for his own use; he had it executed by his domestic, Claude Lechet, whose mechanical talents he had cultivated, as he had before done those of Francis Burnens for natural history. In cases properly numbered, were placed small prominent types which he arranged in his hand. Over the lines thus composed, he placed a sheet blackened with a peculiar ink, then a sheet of white paper, and with a press, which he moved with his feet, he was enabled to print a letter which he folded and sealed himself, happy in the kind of independence which he hoped by this means to acquire.

But the difficulty of putting this press into action prevented the habitual use of it. These letters, and some algebraic characters formed of baked clay, which his ingenious son, always anxious to serve him, had made for his use, were, during more than fifteen years, a source of relaxation and amusement to him. He enjoyed walking, and even a solitary promenade by means of threads which he had caused to be stretched through all the rural walks about his dwelling.

Naturally endowed with a benevolent heart, how were those happy dispositions too often destroyed by the collisions of the world, preserved in him? He received from all that surrounded him nothing but kindness and respect. The busy world, the scene of so many little vexations, had disappeared from his view. His house and his fortune were taken care of, without any embarrassment to him. A stranger to public duties, he was in a great measure ignorant of the politics, the cunning, and the fraud of men. Having rarely had it in his power (without any fault of his own) of being useful to others, he never experienced the bitterness of ingratitude. Jealousy, even notwithstanding his success, was silenced by his infirmity. To be happy and prosperous in a situation in which so many others are given up to continual regrets, was accounted to him as a virtue. The female sex, provided their voices were agreeable, all appeared to him as if he had seen them at the age of eighteen. His mind preserved the freshness and candour which constitute the charm and happiness of adolescence; he loved young people, for with their sentiments his own were more in accordance than with those of the aged and experienced. He took pleasure, to the very last, in directing the studies of the young, and possessed, in the highest degree, the art of pleasing and interesting them. Though fond of new acquaintance, he never abandoned his old friends. "One thing I have never been able to learn," said he in extreme old age, "that is, to forget how to love." Thus had he the good sense justly to appreciate and enjoy the balance of advantages which were furnished him, by the very condition in which he was placed.

His conversation was generally amiable and gracious; he was easily led into the humorous; he was a stranger to no kind of knowledge; he loved to elevate his thoughts to the gravest and most important subjects, as well as to descend to the most familiar sportiveness. He was learned, in the ordinary sense of the word; but, like a skilful diver, he went to the bottom of each question by a kind of tact and a sagacity of perception which supplied the place of knowledge. When any one spoke to him on subjects which interested his head or heart, his noble figure became strikingly animated, and the vivacity of his countenance seemed, by a mysterious magic, to animate even his eyes, which had so long been condemned to darkness.

He spent the last years of his life at Lausanne, under the care of his daughter, Madame de Molin. He continued to make additions at intervals to his former labours. The discovery of bees without stings, made in the environs of Tampico by Captain Hall, excited his curiosity; and it was a high satisfaction to him when his friend, Professor Prevost, procured for him at first a few individuals, and then a hive of these insects. It was the last homage which he rendered to his old friends, to whom he had devoted so many laborious researches, to whom he owed his celebrity, and, what is more, his happiness. Nothing of any importance has been added to their history since his time.

Huber retained his faculties to the last. He was loving and beloved to the end of his days. At the age of eighty-one, he wrote to one of his friends, "There is a time when it is impossible to remain neglectful; it is, when separating gradually from each other, we may reveal to those we love all that esteem, tenderness, and gratitude have inspired us with towards them. * * * I say to you alone," adds he, farther on, "that resignation and serenity are blessings which have not been refused." He wrote these lines on the 20th of December 1831, and on the 22d he was no more; his life became extinct, without pain or agony, while in the arms of his daughter.—*Abridged from "The Life and Writings of Francis Huber," by Professor De Candolle.*

PART I.

LETTER I.

ON THE IMPREGNATION OF THE QUEEN BEE.

Sir,—As you desired me to transmit a written detail of my principal experiments on bees, when I gave you an account of them at Genthod, I hasten to extract the following observations from my journal. Nothing can be more flattering than the interest you take in my researches: therefore, permit me to remind you of your promise to suggest new subjects for investigation.*

Glass hives, constructed after M. de Reaumur's principles, are of a form unfavourable to the observer; because their width allowing the bees to build two combs parallel, whatever passes between them is concealed from his view. Long experience

* All the letters in the first portion of the work are addressed to the celebrated naturalist M. Bonnet, whose labours in this department of science are well known and justly appreciated. His decease in 1793, during the prosecution of the author's experiments, explains the reason for dividing the volume into two parts.—*T.*

of this has induced you to recommend hives much flatter or thinner; the panes of which should be separated by so small an interval, that only a single row of combs could be erected between them. From having felt the same inconvenience, I have profited by your counsel in providing hives reduced to an inch and a half in width, wherein swarms have been established without any difficulty. Here, however, the charge of constructing a single comb must not be committed to the bees: they are taught by nature to make more than one, and all parallel to each other—a law from which they never derogate, unless when constrained by some particular arrangement. Therefore, if left to themselves in these flat hives, which cannot admit of two combs parallel to the plane of the sides, they will form several small ones perpendicular to it; and in that case all will be equally lost to the observer. Thus previous dispositions become essential for the direction of the combs. I so contrived that, while they were built perpendicular to the horizon, the lateral surfaces should be three or four lines from the panes constituting the sides of the hive.* This distance, in allowing sufficient latitude for the motions of the bees, prevented them from collecting in too large clusters on the surface of the comb. By such precautions, they were easily established in very thin hives, where they pursued their labours with the same assiduity and regularity; and every cell being exposed to view, none of their proceedings could be concealed.

* The different measurements are expressed in lines, of which 12 are in an inch.—*T.*

It is true, that by compelling these insects to live in a habitation where they could construct only a single row of combs, I had, in a certain measure, changed their natural condition, and this circumstance possibly might have affected their instinct.* Therefore, to obviate every objection, I invented a kind of hives, which, without losing the advantages of those very thin, at the same time approached the figure of common hives, where bees form several rows of combs.

I procured several small fir frames, a foot square and an inch and a quarter broad; and connecting them together by hinges, the whole, like so many divisions, could be opened and shut as the leaves of a book—Plate 1, fig. 1, 2. When using a hive of this description, we previously fixed a comb in each frame, and then introduced all the bees which were required for the particular experiment. Opening the different divisions in succession, we daily inspected both surfaces of every comb: there was not a single cell where we could not see distinctly whatever passed at all times, nor a single bee, I may almost say, with which we were not particularly acquainted. Indeed this apparatus is nothing more than the union of several very flat hives capable of separation. But bees must not be visited, in such a habitation, before their combs are fixed securely in the frames, otherwise they may kill or maim them by falling out, or excite that degree of irritation as will expose the observer to

* A single comb of very large dimensions, attached under the boughs of a tree, is said to be constructed by a species of bees in India.—*T.*

being stung, which is always painful, and sometimes dangerous: but they soon become accustomed to their situation, and are in some respect tamed by it. In three days we may begin to operate on the hive, to open it, remove part of the combs, and substitute others, without the bees exhibiting too formidable symptoms of displeasure. You will remember, Sir, that on visiting my retreat, I showed you a hive of this kind that had been a long time in experiment, and how much you were surprised that the bees so quietly allowed us to open it.

In these hives I have repeated all my observations, and obtained exactly the same results as in the thinnest. Thus I think already to have obviated any objections that may arise concerning the supposed inconvenience of flat hives. Besides, I cannot regret the repetition of my labours; by going over the same course several times, I am much more certain of having avoided error; and it also appears, that some advantages are found in these which may be called *Book*, or *Leaf-hives*, as they prove extremely useful in the economical treatment of bees, which shall be afterwards detailed.

Having now come to the particular object of this letter, the fecundation of the queen bee, I shall, in a few words, examine the different opinions of naturalists; next, I shall state the most remarkable observations which their conjectures have induced me to make, and then describe the new experiments by which I think I have solved the problem.*

* I cannot insist that my readers, the better to comprehend what is here said, shall peruse the Memoirs of M. de Reaumur on Bees, and those of the Lusatian Society; but I must re-

Swammerdam, who studied bees with unremittin attention, and who could never see the real union of a drone and a queen, was satisfied of its being unneces sary for fecundation of the eggs: but having re marked that, at certain times, the drones exhaled a very strong odour, he thought it an emanation which operated fecundation by penetrating the body of the female. His conjecture was confirmed on dissecting the male sexual organs; for he was so much struck with the disproportion between them and those of the female, that he did not believe their union possible. Besides, his opinion concerning the influence of the odour was plausible, from affording a good reason for the prodigious number of males. There are frequently fifteen hundred or two thousand in a hive; and, according to Swammerdam, it is necessary they should be numerous, that the emanation proceeding from them may have an intensity or energy sufficient to effect impregnation.

Though M. de Reaumur has refuted this hypothesis by just and conclusive reasoning, he has failed to make the sole experiment that could support or overturn it: which was confining all the drones of a hive in a tin case, perforated with minute apertures, that might allow the emanation of the odour to escape, but prevent transmission of their organs. The case should have been then placed in a hive well peopled, but completely deprived of males, both of large and small size, and the consequences observed. It is evi-

quest them to examine M. Bonnet's works, tom. 5, 4to. edit. and tom. 10, 8vo., where they will find a short and distinct abstract of all that naturalists have hitherto discovered on the subject.

dent, had the queen laid eggs after matters were thus disposed, that Swammerdam's hypothesis would have acquired probability; and, on the contrary, it would have been confuted had she produced no eggs, or only sterile ones. However, the experiment has been made by us, and the female remained barren; therefore, it is undoubted, that the emanation of the odour of the males does not impregnate queens.

M. de Reaumur was of a different opinion. He thought that the queen's fecundation followed actual union. Having confined several drones in a glass vessel along with a virgin queen, he saw the females make many advances to the males; but, unable to observe any thing so intimate that it could be denominated their union, he leaves the question undecided. We have repeated this experiment also; we have frequently confined virgin queens with drones of all ages: we have done so at every season, and witnessed all their advances and solicitations towards the males: we have even believed that we saw a kind of union between them, but so short and imperfect that it was unlikely to effect impregnation. Yet, to neglect nothing, we confined to her hive a virgin queen, that had suffered the approaches of the male. During a month that her imprisonment continued, she did not lay a single egg; therefore, these momentary junctions do not accomplish fecundation.

In the *Contemplation de la Nature*, you have cited the observations of the English naturalist Mr Debraw, which, from their apparent accuracy, seemed at last to elucidate the mystery. Favoured by chance, that observer one day perceived, at the bottom of cells containing eggs, a matter apparently prolific, at least very different from the substance or jelly which bees commonly collect around their new hatched worms. Solicitous to learn its origin, and conjecturing that it might be masculine matter, he began to watch the motions of every drone in the hive, on purpose to seize the moment when it should be received by the eggs. He assures us, that he saw several drones insinuate the posterior part of the body into the cells for that purpose. After frequent repetition of the first, he entered on a long series of other experiments. He confined a number of workers in glass bells along with a queen and several males. They were supplied with pieces of comb containing honey, but wanting brood. He saw the queen lay eggs, which were bedewed by the males, and from which larvæ were hatched:* consequently, he could not hesitate in advancing as a fact demonstrated, that male bees fecundate the queen's eggs in the manner of frogs and fishes, that is, after they are produced.†

There was something very specious in this explanation: the experiments on which it was founded seemed correct; and it afforded a satisfactory reason for the prodigious number of males in a hive. At the same time, the author had neglected to obviate one strong objection. Larvæ appear when there are no drones. From the month of September until

* The larva is the worm or caterpillar which comes from the egg of an insect; it then changes into another state called the pupa, chrysalis, or nymph, and, lastly, into the perfect animal. Later naturalists incline to substitute this name in all cases where the worm is not seen under its final aspect.—*T.*

† Philosophical Transactions, Vol. lxvii.

April, hives are generally destitute of males, yet, notwithstanding their absence, the queen then lays fertile eggs. Thus, the prolific matter cannot be required for their impregnation, unless we shall suppose that it is necessary at a certain time of the year, while at every other season it is useless.

To discover the truth, amidst these facts apparently so contradictory, I determined to repeat Mr Debraw's experiments, and to observe more precaution than he himself had done. First, I sought for that matter, which he supposes the prolific, in cells containing eggs. Several were actually found with such an appearance, and, during the first days of observation, neither my assistant nor myself doubted the reality of the discovery. But we afterwards found it an illusion arising from the reflection of the light, for nothing like a fluid was visible, except when the solar rays reached the bottom of the cells. This part is commonly covered by shining fragments of the cocoons of worms successively hatched, and the reflection of the light from these, when much illuminated, produces an illusory effect. We proved it by the strictest examination, for no vestiges of a fluid were perceptible when the cells were detached and cut asunder.

Though the first observation inspired us with some distrust of Mr Debraw's discovery, we repeated his other experiments with the utmost care. On the 6th of August 1787, we immersed a hive,* and, with scrupulous attention, examined all the bees while in the

* Bees may be long immersed in water without destroying life. Reaumur found them recover after nine hours immersion; other animals of analogous species exhibit still more wonderful resurrections.—*T.*

bath. We ascertained that there was no male, either large or small; and having examined every comb, we found neither male nymph, nor worm. When the bees were dry, we replaced the whole, along with the queen, in their habitation, and transported them into my cabinet. They were allowed full liberty; therefore, they flew about, and made their usual collections; but, it being necessary that no male should enter the hive during the experiment, a glass tube was adapted to the entrance, of such dimensions that two bees only could pass at once; and we watched the tube attentively during the four or five days that the experiment continued. We should have instantly observed and removed any male appearing, that the result of the experiment might be undisturbed, and I can positively affirm that not one was seen. However, from the first day, which was the 6th of August, the queen deposited fourteen eggs in the workers' cells; and all these were hatched on the tenth of the same month.

This experiment is decisive; since the eggs laid by the queen of a hive where there were no males, and where it was impossible one could be introduced, since these eggs, I say, were fertile, it becomes indubitable that aspersion with the masculine matter is not required to effect their exclusion.

Though it did not appear that any reasonable objection could be started against such an inference, yet, as I had been accustomed in all my experiments to investigate the most trifling difficulties which could occur, I conceived that Mr Debraw's partisans might maintain, that the bees, deprived of drones, perhaps would search for those in other hives, and carry the fecundative matter to their own habitations for the purpose of depositing it on the eggs.

It was easy to appreciate the force of this objection; for the only thing necessary was repetition of the former experiments, and confinement of the bees so closely to their hives that none could possibly escape. You know very well, Sir, that these animals can live three or four months confined in a hive well stored with honey and wax, if apertures are left for circulation of the air. This experiment was made on the tenth of August; and I ascertained, by means of immersion, that no male was present. The bees were confined four days in the closest manner, and then I found forty young larvæ, recently hatched.

I extended my precautions so far as to immerse the same hive a second time, to be assured that no male had escaped my researches. Each of the bees was separately examined, and none was there that did not display its sting. The coincidence of this experiment with the other, proved that the eggs were not externally fecundated.

In terminating the confutation of Mr Debraw's opinion, I have only to explain what led him into error. He employed queens in his experiments, with whose history he was not acquainted from their origin. When he observed that the eggs produced by a queen confined along with males were fertile, he thence determined that they had been bedewed by the prolific matter in the cells. But, to have rendered his conclusion just, he should have first ascertained that the female was in a virgin state, and this he neglected. The truth is, that, without knowing it, he had used a queen after her commerce with the male. Had he taken a virgin queen, the moment she came from the royal cell, and confined her in his vessels along with drones, the result would have been opposite ; for, even amidst a seraglio of males, this young queen never would have laid, as I shall afterwards prove.

The Lusatian observers, and Hattorf in particular, thought the queen was fecundated of herself, without concourse with the males. I shall here give an abstract of the experiment on which this opinion is founded.*

Hattorf took a queen whose virginity he could not doubt. He excluded all the males of the large and likewise of the small species, and, in several days, found both eggs and worms. He asserts that there were no drones in the hive during the course of the experiment; but, although they were absent, the queen laid eggs, from which worms proceeded: whence he considers that she is impregnated by herself.

Reflecting on this experiment, I did not find it sufficiently accurate. Males pass with great facility from hive to hive; and Hattorf took no precaution against any being introduced into his. He says, indeed, there was no male, but is silent respecting the means adopted to prove the fact: And although he might be satisfied of no large drone being present, still a small one might have escaped his vigilance, and fecundated the queen. With a view to clear up the doubt, I resolved to repeat his experiment, in the manner described by him, and without greater care or precaution.

* Vide in Schirach's History of Bees, a memoir by Hattorf, entitled, *Physical Researches whether the Queen Bee requires Fecundation by Drones?*

I put a virgin queen into a hive, from which all the males were excluded, but the bees left at perfect liberty. Several days afterwards I visited the hive, and found new hatched worms in it. Here then was the same result as Hattorf obtained! But before deducing the same consequence, we had to ascertain beyond dispute that no male had entered the hive. Thus it was necessary to immerge the bees, and examine each separately: By which operation we actually found four small males. Therefore, to render the experiment decisive, not only was it requisite to remove all the drones, but also, by some infallible method, to prevent any from being introduced, which the German naturalist had neglected.

I prepared to repair this omission, by putting a virgin queen into a hive, from which the whole males were carefully removed; and to be physically certain that none could obtain access, a glass tube was adapted at the entrance of such dimensions that the working bees could freely pass and repass, but too narrow for the smallest male. Matters continued thus for thirty days; the workers departing and returning, performed their usual labours; but the queen remained sterile. At the expiration of that time, her belly was equally slender as at the moment of her origin. I repeated the experiment several times, and always with the same result.

Therefore, as a queen, rigorously separated from all commerce with the male, remains sterile, it is evident she cannot impregnate herself, and that Hattorf's opinion is ill-founded.

Hitherto, by endeavouring to confute or verify the conjectures of all the authors who had preceded me, by new experiments, I had acquired the knowledge of new facts, but these were apparently so contradictory as to render the solution of the problem still more difficult. While examining Mr Debraw's hypothesis, I confined a queen in a hive, from which all the drones were removed ; yet she was fertile. When considering the opinion of Hattorf, on the contrary, I put one of whose virginity I was perfertly satisfied in the same situation, she remained sterile.

Embarrassed by so many difficulties, I was on the point of abandoning the subject of my researches, when at length, on more attentive reflection, I thought these contradictions might arise from experiments made indifferently on virgin queens, with whose history I was not acquainted from the origin, and which perhaps had been fecundated unknown to me. Impressed with this idea, I undertook a new method of observation, not on queens fortuitously taken from the hive, but on females decidedly in a virgin state, and whose history was known to me from the instant of their leaving the cell.

From a very great number of hives, I removed all the reigning females, and substituted for each a queen taken at the moment of her birth. The hives were then divided into two classes. All the males, both large and small, were taken from the first, and I adapted a glass tube at the entrance, so narrow that no drone could pass, but large enough for the free passage of the common bees. In the hives of the second class, I left the whole drones belonging to them, and even introduced more ; and to prevent them from escaping, a glass tube, also too narrow for the males, was adapted to their entrance.

For more than a month, I carefully watched this experiment, made on a large scale ; but, much to my surprise, every queen remained sterile. Thus it was proved that queens confined in a hive would continue barren though amidst a seraglio of males.

This result induced me to suspect that the females could not be fecundated in the interior of the hive, and that it was necessary for them to leave it for receiving the approaches of the male. To ascertain the fact was easy, by a direct experiment; and as the point is important, I shall relate in detail what was done by my secretary and myself on the 29th of June 1788.

Aware that in summer the males usually leave the hive in the warmest part of the day, it was natural for me to conclude that if the queens were obliged to go out for fecundation, instinct would induce them to do so at the same time as the others.

At eleven in the forenoon, we placed ourselves opposite to a hive containing an unimpregnated queen five days old. The sun had shone from his rising ; the air was very warm ; and the males began to leave the hives. We then enlarged the entrance of that selected for observation, and paid great attention to the bees entering and departing. The males appeared, and immediately took flight. Soon afterwards, the young queen came to the entrance ; at first she did not fly, but during a little time traversed the board, brushing her belly with her hind legs; neither workers nor males bestowing any notice on her. At last she took flight. When several feet from the hive, she returned and approached it, as if to examine the place of her departure, perhaps judging this precaution necessary to recognise it; she then flew away, describing horizontal circles twelve or fifteen feet above the earth. We contracted the entrance of the hive, that she might not return unobserved, and placed ourselves in the centre of the circles described in her flight, the more easily to follow her, and witness all her motions. But she did not remain long in a situation favourable for our observations, and rapidly rose out of sight. We resumed our place before the hive; and in seven minutes the young queen returned to the entrance of a habitation which she had left for the first time. Having found no external evidence of fecundation, we allowed her to enter. In a quarter of an hour she re-appeared, and, after brushing herself as before, took flight; then returning to examine the hive, she rose so high that we soon lost sight of her. This second absence was much longer than the first; it occupied twenty-seven minutes. We now found her in a state very different from that in which she was after her former excursion; the organs distended by a substance, thick and hard, very much resembling the matter in the vessels of the male; completely similar to it indeed in colour and consistence.*

* It will afterwards appear, that what we took for the generative matter was the male organs left in the body of the female; a discovery which we owe to a circumstance that shall be immediately related. Perhaps I should avoid prolixity, by suppressing all my first observations on the impregnation of the queen, and passing directly to the experiments that prove she carries away the genital organs; but in such observations, which are both new and delicate, and where it is so easy to be deceived, I consider that a candid avowal of my errors is doing the reader service. This is an additional proof to so many others of the absolute necessity that an observer

But more evidence than mere resemblance being requisite to establish that the female had returned with the prolific matter of the males, we allowed this queen to enter the hive, and confined her there. In two days, we found her belly swoln; and she had already laid nearly an hundred eggs in the workers' cells.

To confirm our discovery, we made several other experiments, and with the same success. I shall continue to transcribe my journal.

On the second of July, the weather being very fine, numbers of males left the hives; and we set at liberty a young virgin queen, eleven days old, whose hive had been always deprived of them. Having quickly left the hive, she returned to examine it, and then rose out of sight. She came back in a few minutes without any external marks of impregnation, and departed again in a quarter of an hour, with so rapid a flight that we could scarcely follow her a moment. This absence continued thirty minutes; but on her return, the last ring of the body was open, and the organs full of the whitish substance already mentioned. She was then replaced in the hive, from which all the males were excluded. In two days we found her impregnated.

These observations at length demonstrate why Hattorf obtained results so different from ours. His queens, though in hives deprived of males, had been fecundated; and he thence concludes that sexual intercourse is not requisite for their impregnation. But not having confined the queens to their hives, they

should repeat all his experiments a thousand times to obtain the certainty of seeing facts as they really exist.

had profited by their liberty to unite with the males. We, on the contrary, have surrounded our queens with a number of males, yet they continued sterile; because the precautions for confining the males to their hives had also prevented the queens from departing to seek that fecundation without which they could not obtain within.

The same experiments were repeated on queens twenty, twenty-five, and thirty days old. All became fertile after a single impregnation. However, we have remarked some essential peculiarities in the fecundity of those remaining in the virgin state until the twentieth day of their existence; but we shall defer speaking of the fact until being able to present naturalists with observations sufficiently correct and numerous to merit their attention: yet let me add a few words to what I have already said. Though neither my assistant nor myself have witnessed the commerce of a queen and a drone, we think that, after the detail which has been just commenced, no doubt of the fact can remain, nor can its necessity to effect impregnation be disputed. The sequel of our experiments, made with every possible precaution, appears demonstrative. The uniform sterility of queens in hives wanting males, and in those where they are confined along with them; the departure of these queens from the hives; and the very conspicuous evidence of impregnation with which they return, are proofs against which no objections can stand. But we do not despair of being able next spring to obtain the complement of this proof, by seizing the female at the very moment of her union.

Naturalists always have been extremely embarrassed to account for the number of males in most hives, and which seem only a burden on the community, since they fulfil no function. But we now begin to discern the object of nature in multiplying them to such an extent. As fecundation cannot be accomplished within, and as the queen is obliged to traverse the expanse of the atmosphere, it is requisite the males should be numerous, that she may have the chance of meeting some one of them. Were only two or three in each hive, there would be little probability of their departure at the same instant with the queen, or that they would meet in their excursions; and most of the females thus would remain sterile.*

But why has nature prohibited sexual union within the hives? This is a secret still unknown to us. It is possible, however, that some favourable circumstance may enable us to penetrate it in the course of our observations. Various conjectures may be formed; but at this day we require facts, and reject gratuitous suppositions. It should be remembered that bees do not form the sole republic among insects pre-

* Remarkable irregularity subsists in the number of males, compared with the other inhabitants of a hive. Swammerdam found 693 along with 8494 workers. Previous to the swarming of a large hive, Reaumur counted 700 among 26,426 common bees, and one queen. In another, containing only 2900 workers, he found 693 males. He computed 50,000 cells in the former, of which 20,000 were full of brood. About 2520 cells were appropriated for breeding males, and above half of them were occupied by larvæ and nymphs. Thus, including the 700 in the perfect state, he observes, that this hive would be provided with above 2000 males. Tom. v. p. 561.—*T.*

senting a similar phenomenon; female ants are also obliged to leave the ant-hills previous to fecundation.*

I cannot request, Sir, that you will communicate those reflections which your genius will excite concerning the facts I have related. This is a favour to which I am not yet entitled. But as new experiments unquestionably will occur to you, whether on the impregnation of the queen or on other points, may I solicit you to suggest them? They shall be executed with all possible care; and I shall esteem this mark of friendship and interest as the most flattering encouragement that the continuance of my labours can receive.—*Pregny*, 13*th August*, 1789.

LETTER FROM M. BONNET TO M. HUBER.

You have surprised me most agreeably, Sir, with your interesting discovery of the impregnation of the queen bee. It was a fortunate conjecture that she left the hive to be fecundated; and your method of ascertaining the fact was extremely judicious, and well adapted to the object.

Let me remind you, that male and female ants unite in the air; and that after impregnation the females return to the ant-hills to deposit their eggs. *Contemplation de la Nature, Part. II., chap.* 22, *note* 1. It would be necessary to seize the instant when the drone

* The males and females of ants are winged insects; the former perish some time after their amours, and the females lose their wings a certain period after impregnation.—*T.*

unites with the female. But how remote from the power of the observer are the means of ascertaining their commerce in the air! If you have satisfactory evidence that the matter seen on the last rings of the female is truly masculine, it is more than mere presumption in favour of the truth. Perhaps it may be necessary that the male should seize the female under the belly, which cannot be easily done but in the air. The large opening at the extremity, which you have observed in so particular a condition, seems to correspond with the singular size of the organs of the male.

You wish that I should suggest some new experiments on these industrious republicans. In doing so, I shall take the greater pleasure and interest, as I know to what extent you possess the valuable art of combining ideas, and of deducing from this combination results adapted to the discovery of new facts. A few at this moment occur to me.

It may be proper to attempt the artificial fecundation of a virgin queen with a pencil, at the same time observing every precaution to avoid error. This experiment, you are aware, has already succeeded with more than one animal.

To ascertain that the queen, which has left the hive for impregnation, is the same that returns to deposit her eggs, you will find it necessary to colour the thorax with some varnish resisting humidity. It will be proper also to paint the thorax of a considerable number of workers, in order to discover the duration of their life, which is a more secure method than slight mutilations.

That the worm may be hatched, the egg must be fixed almost vertically by one end near the bottom of the cell. Is it true that it is unproductive, unless when in this position? Unable to determine the fact, I leave it to the decision of experiment.

I formerly mentioned to you that I had long doubted the real nature of the small ovular substances deposited by the queens in the cells, and my inclination to suppose them minute worms not yet begun to expand. Their elongated figure seems to favour my conjecture. It would be expedient, therefore, to watch them with the utmost assiduity from the instant of production until the period of exclusion. If the integument bursts, there can be no doubt that these minute substances are real eggs.

I return to the mode of the union taking place. The height that the queen and the males rise to in the air prevents us from seeing what passes between them; on which account the hive should be put into an apartment with a very lofty ceiling. M. de Reaumur's experiment, confining a queen with several males in a glass vessel, merits repetition; and if, instead of a vessel, a glass tube some inches in diameter and several feet long were used, perhaps something satisfactory might be discovered.

You have had the fortune to observe the small queens mentioned by the Abbé Needham, but which he never saw. It will be of great importance to dissect them for the purpose of finding their ovaries. When Mr Riem informed me that he had confined three hundred workers, along with a comb containing no eggs, and afterwards found hundreds in it, I strongly recommended that he should dissect the workers. He did so; and informed me that eggs were discovered in three. Probably without being aware of it, he has dissected small queens. As small drones exist, it is not surprising if small queens are produced also, and undoubtedly by the same external causes.

It is of much consequence to be intimately acquainted with this species of queens, for they may have great influence on different experiments, and may embarrass the observer: we should ascertain whether they inhabit pyramidal cells smaller than the common or hexagonal ones.

M. Schirach's famous experiment on the supposed conversion of a common worm into a royal one cannot be too often repeated, though the Lusatian observers already have done so frequently. I am anxious to learn whether, as the discoverer maintains, the experiment will succeed only with worms three or four days old, and never with simple eggs.

The Lusatian observers, and the naturalist of the Palatinate, affirm, that when common bees are confined with combs absolutely void of eggs, they then lay none but those of drones. Thus, there may be small queens producing the eggs of males only, for it is evident they must have produced those supposed to come from workers. But how is it possible to conceive that their ovaries contain male eggs alone?

According to M. de Reaumur, the life of chrysalids may be prolonged by keeping them in a low temperature, such as that of an ice-house. The same experiment should be made on the eggs of a queen, and on the nymphs of drones and workers.

Another interesting experiment would be removing all the combs composed by the common cells, and leaving none but those destined for the larvæ of males. By this means we should learn whether the eggs of common worms, laid by the queen in the large cells, will produce large workers. It is very probable, however, that deprivation of the common cells might discourage the bees, because they require them for their honey and wax. Nevertheless, it is likely, that by taking away only part of them, the workers may be forced to lay common eggs in the cells of drones.

I should also wish to have the young larva gently removed from the royal cell, and deposited at the bottom of a common one, along with some of the royal food.

As the figure of hives has much influence on the respective disposition of the combs, it would be a satisfactory experiment greatly to diversify their shape and internal dimensions. Nothing could be better adapted to instruct us how bees can regulate and apply their labours to existing circumstances. This may enable us to discover particular facts which we cannot foresee.

The royal eggs, and those producing drones, have not yet been carefully compared with the eggs from which workers proceed. But it ought to be done, that we may ascertain whether these different eggs have secret distinctive characteristics.

The food supplied by the workers to the royal worm, is not the same with that given to the common worm. Could we not endeavour, with the point of a pencil, to remove a little of the royal food and give it to a common worm deposited in a cell of the largest dimensions? I have seen common cells hanging almost vertically, where the queen had laid; and these I should prefer for such an experiment.

Various facts demanding corroboration were collected in my Memoirs on Bees; of which number are my own observations. You can select what is proper. You have already enriched the history of bees so much, that every thing may be expected from your understanding and perseverance. You know the sentiments with which you have inspired the CONTEMPLATOR OF NATURE.—*Genthod*, 18*th August*, 1789.

LETTER II.

SEQUEL OF OBSERVATIONS ON THE IMPREGNATION OF THE QUEEN BEE.

ALL the experiments, related in my preceding letter, were made in the years 1787 and 1788. They seem to establish two facts, which had been previously the subject of vague conjecture :—1. That the queen bee is not impregnated of herself, but is fecundated by union with the male—2. That this is accomplished without the hive, and in the air.

The latter appeared so extraordinary, that, notwithstanding all the evidence obtained, we eagerly desired to take the queen in the fact ; but, as she constantly rose to a great height, we never could see what passed. On that account you advised us to cut some part off the wings of virgin queens. We endeavoured in every possible manner to benefit by your advice, but, to our great regret, when the wings lost much, the bees could no longer fly ; and by cutting off only an inconsiderable portion, we did not diminish the rapidity of their flight—probably there is a medium, though we were unable to attain it. On your suggestion, also, we tried to render their vision less acute, by covering the eyes with an opaque varnish—an experiment equally fruitless.

We likewise attempted artificial fecundation, and took every possible precaution to insure success, yet the result was always unsatisfactory. Several queens became the victims of our curiosity, and those surviving remained sterile. Notwithstanding these different experiments were unsuccessful, it was proved that queens leave their hives to seek the males, and that they return with undoubted evidence of fecundation. Satisfied with this, we could only trust to time or accident for decisive proof of an actual union. We were far from suspecting a most singular discovery, made by us in July this year, which affords complete demonstration of the supposed event, namely, that the sexual organs of the male remain with the female.*

I conjecture that the males perish after losing their organs; but why so great a sacrifice is exacted by nature involves a mystery which I cannot pretend to unveil. I am unacquainted with any analogous fact in natural history; but as there are two kinds of insects, namely, ephemeræ and ants, whose union seems to take place only in the air, it would be extremely interesting to ascertain whether their males also lose the same parts in similar circumstances, and whether,

* The remainder of this chapter chiefly consists of anatomical details, which may be rather considered an interruption of the narrative. The Translator has judged it expedient to transfer an abstract of them to an Appendix.

as with drones, enjoyment in their flight is the prelude of death.*

* 29th May 1813.—I have not witnessed the sexual commerce of ephemeræ; but were it attended with the mutilation of the males, it would not have escaped M. de Geer, who has done so. In regard to ants, their males are so little liable to such a penalty, that, as I have assured myself by repeated observations, the successive fecundation of several females can follow.

LETTER III.

THE SAME SUBJECT CONTINUED.—OBSERVATIONS ON RETARDING THE FECUNDATION OF QUEENS.

In my first letter I remarked, that when queens were prevented from receiving the approaches of the male until the twenty-fifth or thirtieth day of their existence, the result presented very interesting peculiarities. My experiments at that time were not sufficiently numerous; but they have been since so often repeated, and the result so uniform, that I no longer hesitate to announce, as a certain discovery, the singularities which retarded fecundation produces on the ovaries of the queen. If she receives the male during the first fifteen days of her life, she remains capable of laying the eggs both of workers and of drones; but should impregnation be retarded until the twenty-second day, her ovaries are vitiated in such a manner that she becomes unfit for laying the eggs of workers, and will produce only those of drones.

In June 1787, being occupied in researches relative to the formation of swarms, I had occasion, for the first time, to observe a queen that laid none but the eggs of males. When a hive is ready to swarm, I had previously remarked that the moment of departure is always preceded by a very lively agitation, which first affects the queen, is then communicated to the workers, and excites such a tumult among them, that they abandon their labours, and rush in disorder to the outlets. I then knew very well the cause of the queen's agitation, and it is described in the history of swarms, but I was ignorant how the delirium was communicated to the workers; and this difficulty interrupted my researches. Therefore I thought of investigating, by direct experiments, whether at all times, when the queen was greatly agitated, even not in the time of the hive swarming, her agitation would be communicated in like manner to the workers. The moment one was hatched, I confined her to the hive by contracting the entrances. When assailed by the imperious desire of union with the males, I could not doubt that she would make great exertions to escape, and that the impossibility of accomplishing it would produce a kind of delirium. We had the patience to observe this queen thirty-five days. Every morning about eleven o'clock, when the weather was fine, and the sunshine invited the males to leave their hives, we saw her impetuously traverse every corner of her habitation, seeking to escape. Her fruitless efforts threw her into an extraordinary agitation, the symptoms of which I shall describe elsewhere, and all the common bees were affected by it. As she never was out during this time, she could not be impregnated. At length, on the thirty-sixth day, I set her at liberty. She soon took advantage of it, and was not long of returning with the most evident marks of fecundation.

Satisfied with the particular object of the experiment, I was far from any hopes that it would lead to the knowledge of another very remarkable fact. How great was my astonishment, therefore, on finding that this female, which, as usual, began to lay forty-six hours after being fructified, produced the eggs of drones, but none of workers, and that she continued ever after to lay those of drones only.

At first, I exhausted myself with conjectures regarding so singular a fact; the more I reflected on it, the more did it seem inexplicable. At length, by attentively meditating on the circumstances of the experiment, it appeared that there were two principles, the influence of which I should first of all endeavour to appreciate separately. On the one hand, the queen had suffered long confinement; on the other, her fecundation had been extremely retarded. You know, Sir, that the commerce of these females generally takes place about the fifth or sixth day; but here it had been postponed until the thirty-sixth. That the peculiarity now demonstrated had resulted from confinement, was extremely improbable; for queens, in their natural state, leave their hives only once to seek the males; all the rest of their life they remain voluntary prisoners. Thus it was unlikely that captivity would produce the consequences which I wished to elucidate. At the same time, as it was essential to neglect nothing in a subject so new, I was desirous of ascertaining whether it was owing to the length of confinement, or to retarded fecundation.

Investigating these points was no easy matter. To discover whether captivity, and not retarded fecundation, would vitiate the ovaries, I found it necessary to allow a female to receive the approaches of a male, and also to keep her imprisoned. Now this could not be, for the union of bees never takes place in hives. For the same reason, it was impossible to retard this event, without keeping the queen in confinement. I was long embarrassed by the difficulty. At length I contrived an apparatus, which, though imperfect, nearly fulfilled my purpose.

I put a queen, at the moment of her last metamorphosis, into a hive well stored, and sufficiently provided with workers and males. The entrance was contracted so as to prevent her exit, but allowing free passage to the workers. I also made another opening for the queen, and adapted a glass tube to it, communicating with a cubical glass box eight feet high. Hither she could at all times repair and fly about, enjoying a purer air than was found within the hive; but she could not be fecundated, for though the males flew about within the same bounds, the space was too limited to admit of any union between them. By the experiments related in my first letter, their connection takes place high in the air only; therefore, in this apparatus, I found the advantage of retarding fecundation, while the liberty which the queen now had did not render her situation too remote from the natural state. I watched the experiment fifteen days. Every fine morning the young captive left her hive; she traversed her glass prison, and flew much about, and with great facility. She laid none during this interval, for she had not united with a male. On the sixteenth day I set her at liberty; she left the hive, rose aloft in the air, and soon returned with full evidence of impregnation. In two days she laid the eggs of workers first, and afterwards as many as the most fertile queens.

It thence followed, 1. That captivity had not altered the organs of the queen—2. Fecundation having taken place within the first sixteen days, she produced both species of eggs.

This was an important experiment. It rendered my labours much more simple, by clearly pointing out the method to be pursued: it absolutely precluded the supposed influence of captivity; and left nothing for investigation, but the consequences of retarded fecundation.

With that view I repeated the experiment; but, instead of giving the virgin queen liberty on the sixteenth day, I detained her until the twenty-first. She departed, rose high in the air, was fecundated, and returned. Thirty-six hours afterwards she began to lay; but it was the eggs of males only, and, although very fruitful afterwards, she laid no other kind.

During the remainder of 1787, and the two subsequent years, I occupied myself with experiments on retarded fecundation, and had constantly the same results. It is undoubted, therefore, that when the sexual union of a queen is retarded beyond the twentieth day, only an imperfect impregnation is operated: instead of laying the eggs of workers and males equally, she will lay none but those of males.

I do not aspire to the honour of explaining this remarkable circumstance. When the course of my experiments led me to observe that some queens laid only the eggs of drones, it was natural to investigate the proximate cause of such a singularity; and I ascertained that it arose from retarded fecundation. My evidence is demonstrative, for I can always prevent queens from laying the eggs of workers, by retarding their fecundation until the twenty-second or twenty-third day of their existence. But what is the remote cause of such a peculiarity? or, in other words, why does the delay of impregnation render queens incapable of laying the eggs of workers? This is a problem on which analogy throws no light; nor in all physiology am I acquainted with any fact that bears the smallest similarity.

The problem becomes still more difficult by reflecting on the natural state of things, that is, when fecundation has not been postponed. The queen then lays the eggs of workers forty-six hours after her union, and continues for the subsequent eleven months to produce these alone; and it is only after this period that a considerable and uninterrupted laying of the eggs of drones commences.* When, on the contrary, impregnation is retarded after the twentieth day, the queen begins, from the forty-sixth hour, to lay the eggs of males, and produces no other kind during her whole life. As, in the natural state, she lays the eggs of workers only, during the first eleven months, it is clear that these, and the male eggs, are not indiscriminately mixed in the oviducts. Undoubtedly they occupy a situation corresponding to the principles that regulate her laying—the eggs of workers are first, and those of drones behind them. Farther, it appears that the queen can lay no male eggs until those of workers, occupying the first place in the oviducts, are discharged. Why, then, is this order inverted by re-

* It appears that this period is liable to modification, and that the great epoch of laying the eggs of males may be accelerated or retarded by the state of the atmosphere promoting or impeding the collections of the bees.

tarded impregnation? How does it happen that the whole workers' eggs which the queen ought to lay, if her commerce had been in due time, now wither and disappear, yet do not obstruct the passage of the eggs of drones, which occupy only the second place in the ovaries? Nor is this all. I have satisfied myself that a single union is sufficient to impregnate the whole eggs that a queen will lay in the course of at least two years. I have even reason to think that it impregnates all the eggs that she will lay during her entire life: but I want absolute proof for more than two years. This, which is truly a very admirable fact in itself, renders the influence of retarded fecundation still more difficult to be accounted for. Since a single junction suffices, it is clear that the masculine matter acts from the first moment on all the eggs that the queen will lay in two years. According to your principles, it gives them that degree of *animation* which afterwards effects their successive expansion. Having received the first impressions of life, they grow, they mature, so to speak, until the day they are produced; and as the laws of laying are invariable, because the eggs of the first eleven months are always those of workers, it is evident that those which appear first are also the eggs that come soonest to maturity. Thus, in the natural state, the space of eleven months is necessary for the male eggs to acquire that degree of increment they must have attained when laid. This consequence, which to me seems immediate, renders the problem insoluble. How can the eggs, which should grow slowly for eleven months, suddenly acquire their full expansion in forty-eight hours, when fecundation has been retarded twenty-one days, and by the effect of retardation alone? Observe, I beseech you, that the hypothesis of successive expansion is not gratuitous; it rests on the principles of sound philosophy. Besides, to be convinced that it is well founded, we have only to look at the figures given by Swammerdam of the ovaries of the queen bee. There we see the eggs in that part of the oviducts contiguous to the extremity, much farther advanced and larger than those contained in the parts more remote. Therefore the difficulty remains in full force—it is an abyss wherein I am lost.

The state of certain vegetable seeds, which, although in good preservation, lose the faculty of germination from age, is the only known fact bearing any relation to that now described. The eggs of workers may also retain, but for a very short time, the property of receiving the impressions of the prolific matter; and after this period, which is about fifteen or eighteen days, elapses, they may become so disorganized as to be no longer susceptible of its stimulus. I am sensible that the comparison is very imperfect; besides, it explains nothing, nor does it even put us on the way of making any new experiments. Let me add but one reflection more:—

Hitherto no other effect has been observed from the retarded impregnation of animals, but their becoming absolutely sterile. The first instance of a female still preserving the faculty of engendering males, is presented by the queen bee. But as no fact in nature is unique, it is most probable that the same peculiarity will be also found in other creatures. An extremely curious object of research would be to consider insects in this new point of view: I say *insects*, for I do not conceive that any thing similar will be found in other species of animals. The experiments now suggested will necessarily begin with insects the most analogous to bees—as wasps, humble bees, mason bees, all kinds of flies, and the like. Some experiments might be also made on butterflies : and, perhaps, an animal may be found whose retarded fecundation will be attended with the same effects as that of queen bees. Should the animal be larger, dissection will be more easily accomplished; and we may be able to discover what happens to the eggs when retarded impregnation prevents their expansion. At least, we may hope that some fortunate circumstance will lead to solution of the problem.*

Let us now return to my experiments. In May 1789, I took two queens just after having undergone their last metamorphosis : one was put into a *leaf hive* well provided with honey and wax, and sufficiently peopled by workers and males, and the other into a hive exactly similar, from which all the drones were removed. The entrances of these hives were too confined for the passage of the females and drones, but left perfect freedom to the common bees. After thirty days' imprisonment, the queens being released, they departed and returned impregnated. On visiting the hives in the beginning of July, I found much brood, but wholly consisting of the worms and nymphs of males. Ac-

* The experiments suggested in this paragraph recal a singular reflection of M. de Reaumur. Where treating of oviparous flies, he says, it would not be impossible for a hen to produce a living chicken, if, after fecundation, the eggs she should lay first could be by any means retained twenty-one days in the oviducts.—*Mem. sur. les Insect. tom.* 4. *mem.* 10.

tually there was not a single worker's worm or nymph. Both queens laid without interruption until autumn, and uniformly the eggs of drones, and they ceased as any other queens in the first week of November.

I was much interested to learn what would become of them in the subsequent spring, whether they would resume laying, or if they required new fecundation; and if they did lay, of what species the eggs would be. However, the hives being very weak, I was apprehensive of their perishing during the winter. Fortunately, we were able to preserve them; and from April 1790, the queens recommenced laying, while the precautions we had taken prevented them from receiving any new approaches of the male. Their eggs were still those of drones.

It would have been extremely gratifying to have followed the history of these two females still farther, but, to my great regret, the workers abandoned their hives on the 4th of May, and that same day I found both queens dead. No weevils were in the hive which could disturb the bees, and the honey was still very plentiful; but as, in the course of the preceding year, no workers had been produced, and winter had destroyed many, their numbers were too few in spring for engaging in their wonted labours, and, from discouragement, they deserted their habitation to occupy the neighbouring hives.

In my Journal, I find a detail of many experiments on retarded impregnation—so many, that it would be tedious to transcribe the whole. I may repeat, however, that there was not the least variation in the principle; and that whenever the union of queens was postponed beyond the twenty-first day, the eggs of males only were produced. Therefore, I shall limit my narrative to those experiments that have taught me some remarkable facts.

A queen being hatched on the 4th of October 1789, we put her into a leaf-hive. Though the season was well advanced, considerable numbers of males were still in it ; and here it became important to learn, whether at this period of the year they could equally effect fecundation ; also, in case it succeeded, whether the queen's laying, begun in the middle of autumn, would be interrupted or continued during winter. Thus, we allowed her to leave the hive. She departed, indeed, but made four-and-twenty fruitless attempts before returning with the evidence of fecundation. Finally, on the thirty-first of October she was more fortunate. Departing, she brought back undoubted proof of the success of her amours ; and now being twenty-seven days old, fecundation had been retarded. She ought to have begun laying within forty-six hours, but the weather was cold, and she laid none, which proves, as we may cursorily remark, that refrigeration of the atmosphere is the principal agent that suspends the laying of queens during winter. I was excessively impatient to learn whether, on the return of spring, she would prove fertile, without a new union. The means of ascertaining the fact was easy, for the entrances of the hives only required contraction, so as to prevent her from escaping. She was confined from the end of October until May. In the middle of March, visiting the combs, we found a considerable number of eggs, but none being yet hatched it could not be known whether they would produce workers or males. On the fourth of April, however, having again examined the state of the hive, we found a prodigious quantity of nymphs and worms, all of drones; nor had this queen laid a single worker's egg.

Here, as well as in the preceding experiment, retardation had rendered the queens incapable of laying the eggs of workers. But this result is the more remarkable, as the queen did not commence laying until four months and a half after fecundation. It is not rigorously true, therefore, that the term of forty-six hours elapses between the sexual union of the female and her laying—the interval may be much longer, if the weather grows cold. Lastly, it follows, that although cold will retard the laying of a queen impregnated in autumn, she will begin to lay in spring without requiring commerce anew.

It may be added, that the fecundity of the queen, whose history is given here, was astonishing. On the first of May, we found in her hive, besides six hundred males already in the winged state, two thousand four hundred and thirty-eight cells, containing either eggs or nymphs of drones. Thus she had laid more than three thousand male eggs during March and April, which is above fifty each day. Her death occurring soon afterwards unfortunately interrupted my observations. I intended to calculate the total number of male eggs that she would have laid throughout the year, and compare it with the laying of queens whose fecundation had not been retarded. You know, Sir, that the latter lay about two thousand male eggs in spring; and another laying, but less considerable, is resumed in August; also that in the interval they produce the eggs of workers almost exclusively. But it is otherwise with the females whose union has been retarded—they produce no workers' eggs. For four, five, or six months following they lay the eggs of males without interruption, and in such numbers, that, in this short time, I suppose one queen gives birth to more drones than are produced in the course of two years by a female whose fecundation has not been retarded. It gives me much regret that I have not been able to verify this conjecture.

I should also describe the very remarkable manner in which queens, laying only the eggs of drones, sometimes deposit them in the cells. Instead of being placed in the lozenges forming the bottom, they are frequently fixed on the lower side of the cells, two lines from the mouth; which results from the body of such queens being shorter than that of those whose fecundation has not been retarded. The extremity remains slender, while the first two rings next the thorax are uncommonly enlarged. Thus, in disposing themselves for laying, the extremity cannot attain the bottom of the cells on account of the swoln rings, consequently the egg must remain attached to the part reached by it. The worms proceeding from the eggs pass their vermicular state in the same place where they are deposited, which proves that bees are not charged with the care of transporting the eggs, as has been supposed. But here another plan is followed; they advance those cells wherein they observe eggs two lines from the orifice, beyond the surface of the comb. It is not essential, however, that they shall be fixed by one end at the bottom of the cell.

Permit me, Sir, to digress a moment from the subject, and give the result of an experiment which seems interesting:—Bees, I say, are not charged with the care of transporting into cells the eggs misplaced by the queen; and, judging by the single instance I have related, you will think me well entitled to deny this feature of their industry. Yet, as several authors have maintained the reverse, and even demanded our admiration of them in conveying the eggs, I should explain clearly that they are deceived.

I had a glass hive constructed of two stages, the higher was filled with combs composed of large cells, and the lower with those of common ones. A kind of division, or diaphragm, separated these two stages from each other, having an opening at each side for the passage of the workers from one stage to the other, but too narrow for the queen. I put a considerable number of bees into this hive, and, in the upper part, confined a very fertile queen that had just finished her great laying of male eggs, therefore, she had only those of workers to lay, and she was obliged to deposit them in the surrounding large cells from the want of others. My object in this arrangement will be already anticipated. The reasoning was simple. If the queen laid workers' eggs in the large cells, and the bees were charged with transporting them if misplaced, they would infallibly take advantage of the liberty allowed them to pass from either stage; they would seek the eggs deposited in the large cells, and carry them down to the lower stage containing the cells adapted for that species. If, on the contrary, they left the common eggs in the large cells, I should obtain certain proof that they were not entrusted with transporting them.

The issue of this experiment excited my curiosity extremely, and we observed the queen several days without intermission. During the first twenty-four hours, she abstained from laying a single egg in the surrounding cells; she examined them one after another, but passed on without insinuating her belly into any. She restlessly traversed the combs in all directions—her eggs appeared an oppressive burden, but she persisted in retaining them rather than they should be deposited in cells of unsuitable diameter. The bees, however, did not cease to pay her homage, and treat her as a mother. It was amusing to observe, when reaching the edges of the division separating the two stages, how she gnawed at them to enlarge the passage; the workers approached her, and also, labouring with their teeth, made every exertion to augment the entrance to her prison, but ineffectually. On the second day, the queen could no longer retain her eggs—they escaped in spite of her, and fell at random, yet eight or ten were seen in the cells which next day had disappeared. Then we conceived that the bees had conveyed them into the small cells of the lower stage, where we sought them with the utmost assiduity, but I can safely affirm not one was there. The eggs that the queen still laid on the third day, disappeared as the first. We again sought them in the small cells, but none were found. The fact is, they were ate by the workers; and this is what has deceived the naturalists, who supposed them carried away. They have remarked the disappearance of the misplaced eggs, and, without farther investigation, have asserted that the bees convey them elsewhere; they take them, indeed, not to be carried off, but to be devoured.*

* This is not peculiar to the workers of these animals. Among humble bees, which also live in society, the workers

Thus Nature has not entrusted bees with the care of placing the eggs in the cells appropriated for them, but she has inspired females themselves with sufficient instinct to know the species of eggs which they are about to lay, and to deposit them in those suitable. This has been already observed by M. de Reaumur, and here my observations correspond with his. Therefore it is certain, that in the natural state, when fecundation ensues at the proper time, and the queen has suffered from nothing, she is never mistaken in the choice of the cells where her eggs are to be deposited; she never fails to lay those of workers in small cells, and those of males in large ones. The distinction is important, for the same unerring instinct is no longer conspicuous in the conduct of females whose impregnation has been deferred. I was oftener than once deceived respecting the eggs laid by such queens, for they were deposited indiscriminately in small cells and in those of drones; and, not aware of their instinct being affected, I conceived that the eggs in small cells would produce workers; whence I was very much surprised to see the bees close up the cells at the moment they should have been hatched, and demonstrate, by anticipation, that the included worms would change into drones. They actually became males—those produced in small cells were small, those in large cells large. Thus I must warn observers, who would repeat my experiments on queens laying only the eggs of males, not to be misled by these circum-

endeavour to destroy the eggs; and after the females have deposited them in the cells, they have to contend vigorously with the common bees for their preservation.—*T.*

stances, and expect that eggs of males will be deposited in the workers' cells.

It is a singular fact also, that females, whose fecundation has been retarded, sometimes lay the eggs of males in royal cells. I shall prove, in the history of swarms, that immediately when queens, in the natural state, begin their great laying of male eggs, numerous royal cells are constructed. Undoubtedly there is some secret relation between the appearance of these eggs and their formation, for bees never derogate from this law of nature. It is not surprising, therefore, that such cells are constructed in hives governed by queens laying the eggs of males only: it is no longer extraordinary that these queens deposit in the royal cells eggs of the only species they can lay, for in general their instinct seems affected. But what I cannot comprehend is, why the bees take exactly the same care of the male eggs deposited in royal cells, as of those that should become queens. They provide them more plentifully with food, they build up the cells as if containing a royal worm—in a word, they labour with such regularity that we have been frequently deceived. More than once, in the firm persuasion of finding royal nymphs, we have opened the cells after they were sealed, yet the nymph of a drone always appeared. Here the instinct of the workers seems defective. In the natural state, they can accurately distinguish the male worms from those of common bees, as they never fail to give a particular covering to the cells containing the former. Why, then, can they no longer distinguish the worms of drones, when deposited in the royal cells? The fact deserves much attention. I am convinced, that, in investigating the instinct of animals, we must carefully observe where it appears to err.*

Perhaps I should have begun this letter with an abstract of the observations of prior naturalists, on queens laying none but the eggs of males; however, I shall here repair the omission.

In a work, *Histoire de la Reine des Abeilles*, translated from the German by Blassiere, there is printed a letter from Schirach to you, dated April 15, 1771, where he speaks of certain hives the whole brood of which changed into drones. You will remember that he ascribes this circumstance to some unknown vice in the ovaries of the queen; but he was far from suspecting that it resulted from retarded fecundation. He justly felicitated himself on discovering a method of preventing the destruction of hives in this condition, which was simple, for it consisted in removing the queen that laid the eggs of males only, and substituting one whose ovaries were not impaired for her. But to render the substitution effectual, it was necessary to procure queens at pleasure—a secret reserved for Schirach, and of which I shall speak in the following letter. You observe that the whole experiments of the German naturalist tended to the preservation of the hives whose queens laid none except male eggs; and that he did not attempt to discover the cause of the evident vitiation of their ovaries.

M. de Reaumur also says a few words somewhere of a hive containing many more drones than workers,

* It is proved by the author's later observations, that the instinct of the workers is not so defective as he is inclined here to conclude.—*T.*

but advances no conjectures on the cause. However, he adds, as a remarkable circumstance, that the males were tolerated in this hive until the subsequent spring. It is true that bees governed by a queen laying only male eggs, or by a virgin queen, preserve their drones several months after they have been massacred in other hives, for which I can ascribe no reason; but it is a fact that I have witnessed several times during my long course of observations on retarded impregnation. In general it has appeared, that while the queen lays male eggs, bees do not destroy the males already perfect in the hive.—*Pregny*, 21*st August*, 1791.

LETTER IV.

ON SCHIRACH'S DISCOVERY.

When you found it necessary, Sir, in the new edition of your works, to give an account of Schirach's beautiful experiments on the conversion of common worms into royal ones, you invited naturalists to repeat them. Indeed, such an important discovery required the confirmation of several testimonies, and for this reason I hasten to inform you that all my researches establish its truth. During ten years that I have studied bees, I have repeated Shirach's experiment so often, and with such uniform success, that I can have no longer the least doubt on the subject. Therefore, I consider it an established fact, that when bees lose their queen, while several workers' worms are preserved in the hive, they enlarge some of their cells, and supply them not only with a different kind of food, but with a greater quantity of it, and that the worms reared in this manner, instead of changing to common bees, become real queens. I request my readers to reflect on the explanation you have given of so uncommon an incident, and the philosophical consequences deduced from it, *Contemplation de la Nature, part* 11, *chap.* 27.

Here I shall content myself with some account of the figure of the royal cells constructed by bees around the worms destined for the royal state, and terminate with discussing some points wherein my observations are at variance with those of Schirach.

Bees soon become sensible of having lost their queen, and in a few hours commence the labour necessary to repair their misfortune. First, they select the young common worms, which, by a certain treatment, are to be converted into queens, and immediately begin enlarging the cells where they are deposited. Their mode of proceeding is curious; and the better to illustrate it, I shall describe the labour bestowed on a single cell, which will apply to all the rest containing worms destined for queens.

The bees having selected a worm, three of the contiguous cells are sacrificed: they supply it with food, and surround it by a cylindrical enclosure, rendering the cell a perfect tube, with a rhomboidal bottom, for this part remains untouched.* Were it injured, three corresponding cells on the opposite surface of the comb would be laid open, and the worms would perish; therefore, their destruction being unnecessary, Nature has opposed it. Leaving the bottom of the cell rhomboidal, the bees are content with raising a cylindrical tube around its worm, which, like the other cells of the comb, is horizontal. But this habitation is adapted to the larva called to the royal state only for the first three days of its existence: it requires another position for the other two

* The bottom of a cell consists of three lozenges or rhomboids, as is amply explained in the second part of the work.—*T.*

that it is still a worm. During these which constitute so small a portion of its life, it must occupy a cell nearly of a pyramidal figure, hanging in a perpendicular direction. We may affirm that the workers are aware of this necessity, for, after the worm has completed the third day, they prepare the place to be occupied by its new lodging. Gnawing away the cells environing the cylindrical tube, they mercilessly sacrifice their worms, and employ the wax in constructing a new pyramidal tube, which they solder at right angles to the first, and work it downwards. The diameter of this pyramid decreases insensibly from the base, which is very wide, down to the point. During the two days that it is inhabited by the worm, a bee constantly keeps its head more or less inserted into the cell, and, on quitting it, another comes to occupy its place. In proportion as the worm grows, the bees labour in extending the cell, and bring food, which they place before its mouth, and around its body. The worm, which can move only in a spiral direction, turns incessantly to take the food before its head; it insensibly descends, and at length arrives at the orifice of the cell. Now is the time of transformation to a nymph. As any farther care would be unnecessary, the bees close the cell with a peculiar substance appropriated for it, and there the worm undergoes both its metamorphoses.

Though Schirach supposes that none but worms three days old are selected for the royal treatment, I am certain of the contrary, and that the process succeeds equally well with those of two days only. I must be permitted to relate at length the evidence I have of the fact, which will demonstrate both the reality of common worms being converted into queens, and the little influence which their age has on the effect of the treatment.

I put some pieces of comb containing workers' eggs in the cells, of the same kind as those already hatched, into a hive deprived of the qeeen. The same day several cells were enlarged by the bees, and converted into royal cells, and the worms supplied with a thick bed of jelly. Five were then removed by us from these cells, and five common worms, which had come from the egg forty-eight hours previously before us, were substituted for them. The bees did not seem aware of the change—they watched over the new worms as over those chosen by themselves. They continued enlarging the cells, and closed them at the usual time. When they had hatched on them seven days,* we took away the cells to preserve the queens that would be produced. Two were excluded, almost at the same moment, of the largest size, and well formed in every respect. The term of the other cells having elapsed without any queen appearing, we opened them, and found in one a dead queen, but still a nymph; the other two were empty. After spinning their silk cocoons, the worms had died before passing into their nymphine state, and presented only a dry skin. I can conceive nothing more conclusive than this experiment; it demonstrates that bees have the power of converting the worms of workers into queens, since they succeeded in procuring them by operating on the worms which we ourselves had selected. It is equally demonstrated, that the success of the process

* For the author's meaning in this passage, see p. 59, last line.— *T.*

does not depend on the worms being three days old, as those entrusted to the bees were only two. Nor is this all—bees can convert worms still younger into queens, for the following experiment shows, that, when the queen is lost, they destine worms only a few hours old to replace her.

A hive in my possession having been long deprived of the female, had neither egg nor worm. I provided a queen of the greatest fertility, and she immediately began laying in the cells of workers; but I removed her before being quite three days in the hive, and before any of her eggs were hatched. The following morning—that is, on the fourth day—we counted fifty minute worms, the oldest scarcely hatched twenty-four hours. However, several were already destined for queens, which was proved by the bees depositing around them a much more abundant provision of food than is supplied to common worms. Next day, the worms being nearly forty hours old, the bees had enlarged and converted their hexagonal cells into cylindrical ones of the greatest capacity. During the subsequent days they still laboured at them, and closed them on the fifth from the origin of the worms. Seven days after sealing of the first of these royal cells, a queen of the largest size issued from it, and immediately rushing towards the other royal cells, she endeavoured to destroy their nymphs and worms. In another letter I shall relate the effects of her fury.

From these details you will observe, Sir, that Schirach's experiments had not been sufficiently diversified when he affirmed that it was essential, for the conversion of common worms into queens, they should be three days old. It is undoubted that equal success attends the experiment, not only with worms two days old, but also when they have been only a few hours in existence.

After my researches to corroborate Schirach's discovery, I was desirous of learning whether, as this observer conceives, the only means which the bees have of procuring a queen is giving the common worms a certain kind of aliment, and rearing them in the largest cells. You will remember that M. de Reaumur's sentiments are very different :—" The mother should lay, and she actually lays, eggs from which bees fit for being mothers must in their turn proceed. She does so, and it is evident the workers know what she is to do. Bees, to which the mother is so precious, seem to take a peculiar interest in the egg that is to produce a mother, and to consider it of the greatest value. They construct particular cells where such are to be deposited. The figure of a royal cell only begun, very much resembles a cup, or, more correctly speaking, the cup that has lost its acorn."

M. de Reaumur, though not suspecting the possibility of a common worm being converted into a queen, conceived that the queen bee laid a particular species of eggs in the royal cells, from which worms should come that would be queens. According to Schirach, on the other hand, bees always having the power of procuring a queen, by bringing up worms three days old in a particular manner, it would be needless for Nature to grant females the faculty of laying royal eggs. Such prodigality is, in his eyes, inconsistent with her ordinary laws ; therefore he maintains, in direct terms, that the queen does not lay royal eggs in cells purposely prepared to receive them. He considers the royal cells only as common ones, enlarged by the bees at the moment when the included worm is destined for a queen, and adds, that the royal cell is always too long for the belly of the mother to reach its bottom.

I admit that M. de Reaumur nowhere says he has seen the queen lay in the royal cell. However, he did not doubt the fact; and, after all my observations, I must esteem his opinion just. It is quite certain that, at particular periods of the year, bees prepare royal cells; that the females deposit their eggs in them; and that worms, which will become queens, issue from these eggs.

Schirach's objection concerning the length of the cells proves nothing, for the queen does not postpone depositing her eggs till they are finished. While only sketched and shaped like the cup of an acorn, she lays in them. This naturalist, dazzled by the brilliancy of his discovery, saw but part of the truth. He was the first to find out the resource granted to bees by Nature for repairing the loss of their queen, and too soon persuaded himself that she had provided none other for the production of females. His error arose from not observing bees in very flat hives. Had he used such as mine, he would have found a confirmation of M. de Reaumur's opinion on opening them in spring. Then, which is the season of swarming, hives in good condition are governed by a very fruitful queen. There are royal cells of a figure widely different from those constructed around the worms destined by the bees for queens—large, attached to the comb by a stalk, and hanging vertically like stalactites, such, in short, as described by M. de Reaumur. The females lay in them before completion, and we have surprised a queen depositing the egg when the cell was only as the cup of an acorn. The workers never lengthen them until it has been laid. In proportion as the worm grows they are enlarged, and closed by the bees when the first transformation approaches. Thus, it is true that in spring the queen deposits eggs in royal cells previously prepared, from which winged insects of her own species are to come. Nature has therefore provided a twofold means for the multiplication and conservation of their race.—*Pregny*, 24*th August* 1791.

LETTER V.

EXPERIMENTS PROVING THAT THERE ARE SOMETIMES COMMON BEES WHICH LAY FERTILE EGGS.

The singular discovery of Riem, concerning the existence of fertile workers, has appeared very doubtful to you, Sir. You have suspected that the eggs ascribed to workers by this naturalist were actually produced by small queens, which, on account of their size, had been confounded with common bees; but you do not positively insist that Riem is deceived; and, in the letter which you did me the honour to address to me, you requested me to investigate, by new experiments, whether there are truly working bees capable of laying fertile eggs. I have made such experiments with great care, and it is for you to judge of the confidence which they merit.

On the fifth of August 1788, we found the eggs and worms of large drones in two hives, both of which had been some time deprived of queens. We also observed the rudiments of several royal cells appended like stalactites to the edges of the combs, and containing the eggs of males. Being perfectly certain that there was no queen of large size among the bees of these two hives, it was evident to us that the eggs, daily becoming more numerous, were laid, either by queens of small size, or by fertile workers. I had reason to believe that it was actually by common bees, for we had frequently observed them inserting the posterior part of the body into the cells, and assuming the same attitude as the queen when laying. But, notwithstanding every exertion, we never had been able to seize one in this position to examine it more narrowly; and we were unwilling to assert any thing positively, without having the bees in our hands that had actually laid. Therefore our observations were continued with equal assiduity, in hopes that, by some fortunate chance, or in a moment of address, we could secure one of them. More than a month all our endeavours were abortive.

My assistant then offered to perform a task requiring both courage and patience, and which I could not resolve to suggest, though the same expedient had occurred to myself. He proposed to examine each bee in the hive separately, in order to discover whether some small queen had not insinuated herself among them, and had escaped our first researches. This was an important experiment, for, should no small queen be found, it would be demonstrative evidence that the eggs had been laid by simple workers.

To accomplish this investigation with all possible exactness, immersing the bees was not enough. You know, Sir, that the contact of water stiffens their organs, that it produces a certain alteration of their external figure; and, from the resemblance of small queens to workers, the slightest alteration of shape would have prevented us from distinguishing with sufficient accuracy to which species those that were immersed might belong. Therefore it was necessary to seize the whole bees of both hives, notwithstanding their irascible nature, and examine their specific character with the utmost care. This my assistant undertook, and executed with great address. Eleven days were employed in it, and, during all that time, he scarcely allowed himself any relaxation, but what the relief of his eyes required. He took every bee in his hand—he attentively examined the trunk, the hind limbs, and the sting. Not one appeared without the characteristics of the common bee, that is, the little basket on the hind legs, the long trunk, and the straight sting. Having previously prepared glass cases containing combs, he put each bee into them after examination. It is superfluous to say they were confined, which was a precaution indispensable, until termination of the experiment. Neither was it enough to establish that the whole were workers; we had also to protract the experiment, and observe whether any of them would produce eggs. Thus we inspected the cells for several days, wherein we soon discovered new laid eggs, from which the worms of drones came at the proper time. My assistant held in his hands the bees that produced them, and, as he was perfectly certain they were common ones, it is proved that there are sometimes fertile workers in hives.

Having corroborated Schirach's discovery by so decisive an experiment, we replaced all the bees examined in flat hives, only an inch and a half wide, which, being capable of admitting but a single row of combs, were very favourable to the observer. We expected, by persisting in strictly watching them, that a fertile one might be surprised in the act of laying, which we were desirous of doing, for the purpose of ascertaining the difference of its ovaries from those of queens by dissection.

At length, on the eighth of September, we had the good fortune to succeed. A bee appeared in the position of a female laying—before having time to leave the cell, we suddenly opened the hive and seized it. This insect presented all the external characteristics of common bees; the only difference we could recognize, and that a very slight one, consisted in the belly seeming less, and more slender than that of workers. On dissection, the ovaries were found to be double, like those of queens, but more fragile, smaller, and composed of fewer oviducts. The filaments containing the eggs were extremely fine, and exhibited swellings at equal distances. We counted eleven eggs of sensible size, some of which appeared to have come to maturity.

On the ninth of September, we seized another fertile worker the instant it laid, and dissected it. The ovary was still less expanded than that of the preceding bee, and only four eggs had attained maturity. My assistant, extracting one from the oviducts, succeeded in fixing it by an end on a glass slider. We may take this opportunity of remarking, that it is in the oviducts themselves the eggs are imbued with the viscous liquid with which they are produced, and not in passing from the spherical sac, as Swammerdam believed.* During the remainder of this month, we

* It is not easy to understand this passage without resorting to delineations of the ovaries, which the reader will find in Swammerdam's Biblia Naturæ, pl. 19, and pl. 32, tom. 5, of Reaumur's Memoirs. A duct leads from each ovary into a

found ten fertile workers in the same hives, and dissected them all. The ovaries of the greater part were easily distinguished, but in some we could not discern the faintest traces of them. In the latter, the oviducts to all appearance were but imperfectly developed, and more address than we had acquired in dissection was necessary for their discovery.

Fertile workers never lay the eggs of common bees; they produce none but those of males. Riem had already ascertained this singular fact; and here all my observations correspond with his. I shall only add to what he says, that fertile workers are not absolutely indifferent to the choice of the cells for depositing their eggs. They always prefer large ones, and resort to small cells but when unable to find those of greater diameter. However, they so far correspond with queens whose impregnation has been retarded, that they sometimes lay in royal cells.

When speaking of females laying male eggs alone, I have already expressed my surprise that bees bestow such care and attention on those deposited in royal cells, as to feed their worms, and, at the period of transformation, to close them up. But I know not, Sir, why I omitted to remark, that after sealing the royal cells, the workers build them up, and sit on them

canal, through which the eggs are transmitted from the queen. From this common canal, a spherical sac rises near the union of the two ducts, where the secretion of some particular fluid was supposed. The eggs of most insects have a kind of viscosity, by means of which, perhaps, they are attached to the places where deposited. Thus, we observe many rows lying horizontally, or fixed by one end on the leaves of plants, and adhering there until the worm is hatched.—*T.*

until the last metamorphosis of the included male.* The treatment of the royal cells where fertile workers lay the eggs of drones is very different. They begin indeed with bestowing every care on their eggs and worms; they close the cells at a suitable time, but never fail to destroy them three days afterwards.

Having finished these first experiments with success, I had still to discover the cause of the evolution of the sexual organs of fertile workers. Riem had not engaged in this interesting problem; and at first I dreaded that I should have no other guide towards its solution than mere conjecture. Yet, on serious reflection, it appeared that some light to direct my progress in this new research might be obtained, from connecting the preceding facts.

I reasoned thus:—From Schirach's elegant discoveries, it is beyond all doubt that common bees are originally of the female sex.† They have derived from Nature the germs of an ovary, but she has admitted of its expansion only in the particular case of their receiving a certain aliment while a worm. Thus it must be the peculiar object of inquiry, whether the fertile workers obtain that aliment while worms.

All my experiments convince me that bees, capable

* It is difficult to discover whether the author thinks, as some naturalists do, that bees are instrumental in hatching the eggs.—*T.*

† It is said by Warder, a practical observer in the earlier part of last century, "After great pains taken, and curious observations made, with the use of the best glasses, I must come to this certain conclusion, that all the working bees are females."—*The True Amazons, or Monarchy of the Bees,* chap. 1.—*T.*

of laying, are produced in hives that have lost the queen. A great quantity of royal jelly is then prepared for feeding the larvæ destined to replace her. Therefore, if fertile workers are produced under these circumstances alone, it is evident their origin is only in those hives where bees prepare the royal jelly, and towards this I must bend all my attention. It induces me to suspect, that when bees give the *royal treatment* to certain worms, they either by accident or by a particular instinct, the principle of which is unknown to me, drop some particles of royal jelly into cells contiguous to those containing the worms destined for queens. The larvæ of workers that have accidentally received portions of so active an aliment, must be more or less affected by it, and their ovaries should acquire a degree of expansion. But this expansion will be imperfect; why? because the royal food has been administered only in small portions, and, besides, the larvæ having lived in cells of the smallest dimensions, their parts cannot enlarge beyond the ordinary proportions. Thus, the bees produced from them will resemble common workers in size, and in all the external characteristics. Added to that, they will have the faculty of laying some eggs, solely from the effect of the trifling portion of royal jelly mixed with their aliment.

That we may judge of the truth of this theory, it is necessary to consider fertile workers from their origin; to investigate whether the cells, where they are brought up, are constantly in the vicinity of the royal cells; and if their food be mixed with particles of the royal jelly. Unfortunately the execution of these experiments is very difficult. When pure, the royal jelly is recognised by its sharp and pungent taste; but when mixed with other substances, its peculiar savour is imperfectly distinguished.

Thus I conceived that my investigation should be limited to the situation of the cells; and, as the subject is important, permit me to enter a little into detail.*

In June 1790, I observed that one of my thinnest hives had wanted the queen for several days, and that the bees had no means of replacing her, there being no workers' worms. I then provided them with a small portion of comb, each cell containing a young worm of the working species. Next day, the bees prolonged several cells around the worms destined for queens into the form of royal ones. They also bestowed some care on the worms in the adjoining cells. Four days afterwards, all the royal cells were shut, and we counted nineteen small cells also perfected, and closed by a covering almost flat. In these were worms that had not received the royal treatment; but as they had lived in the vicinity of the worms destined for replacing the queens, it was very interesting to follow their history, and essential to watch the moment of their last transformation. I removed the nineteen cells into a grated box, which was introduced among the bees. I also removed the royal cells, for it was of great importance that the queens they would produce should not disturb or derange the result of the experiment. But here another precaution was also requisite. It was to be dreaded that the bees, being deprived of the produce of their labour and the object

* The original is extremely confused in the preceding passages.—*T.*

of their hope, might be totally discouraged; therefore, I supplied them with another piece of comb, containing the brood of workers, having it in my power to destroy the young when necessary. This plan succeeded admirably. The bees, in bestowing all their attention on these latter worms, forgot those that had been removed.

When the moment of transformation of the nymphs in the nineteen cells arrived, I examined the grated box frequently every day, and at length found six bees exactly similar to *common bees*. The worms of the remaining thirteen had perished without changing.

The portion of brood comb that had been brought into the hive to prevent the discouragement of the bees was then removed. I put aside the queens produced in the royal cells; and having painted the thorax of the six bees red, and amputated the right antenna, I transferred the whole into the hive, where they were well received.

You easily conceive my object, Sir, in this course of observations. I knew there was neither a large nor small queen in the hive: therefore, if, in the sequel, I should find new laid eggs in the combs, how very probable must it be that they had been produced by some of the six bees? But, to attain absolute certainty, it was necessary to take them in the act of laying. Some ineffaceable mark was also required for distinguishing them in particular.

These proceedings were attended with the most ample success. We soon found eggs in the hive; their number increased daily; and their worms were all drones. But a long interval elapsed before we could take the bees that laid them. At length, by means of assiduity and perseverance, we perceived one introducing the posterior part into a cell; we opened the hive, and caught the bee. We saw the egg it had deposited, and, by the colour of the thorax and privation of the right antenna, instantly recognised it as one of the six that had passed the vermicular state in the vicinity of the royal cells.

I could no longer doubt the certainty of my conjecture; at the same time, I know not whether the truth will appear as rigorous to you, Sir, as it does to myself. But I reason in the following manner:—If it be certain that fertile workers are always produced in the vicinity of royal cells, it is no less true, that in itself that vicinity is indifferent, for the size and figure of these cells can have no effect on the worms in those surrounding them—there must be something more. We know that a particular aliment is conveyed to the royal cells; we also know, that this aliment has a very powerful effect on the ovaries, that it alone can unfold the germ. Thus we must necessarily suppose, that the worms in the adjacent cells have had a portion of the same food—and this is what they gain by vicinity to the royal cell. Numbers of bees, in their course thither, will pass over them, stop and drop some portions of the jelly destined for the royal larvæ. Such reasoning, I presume, is consistent with the principles of sound logic.

I have repeated the experiment now described so often, and weighed all the concomitant circumstances with so much care, that whenever I please I can obtain fertile workers in my hives. The method is simple. I remove the queen from a hive; and very soon the bees labour to replace her, by enlarging several cells containing the brood of workers, and by supplying the included worms with the royal jelly. Portions of the same aliment also fall on the young larvæ deposited in the adjacent cells, and occasion a certain evolution of the ovaries. Fertile workers are constantly produced in hives where the bees labour to replace their queen, but we very rarely find them, because they are attacked and destroyed by the young queens reared in the royal cells. Therefore, all their enemies must be removed for their preservation, and the larvæ of the royal cells taken away before undergoing their last metamorphosis. Then the fertile workers, being free of rivals at the time of their origin, will be well received; and, by taking the precaution of marking them, it will be seen, in a few days, that they produce the eggs of males. Thus, the whole secret of this process consists in removing the royal cells at the proper time, that is, after being sealed, and previous to the young queens leaving them.*

I shall add but a few words to this long letter. There is nothing so very surprising in the production of fertile workers, when we attend to the consequences of Schirach's beautiful discovery—but why do they lay male eggs only? I can conceive, indeed, that the

* I have frequently seen queens, at the moment they are produced, begin first by attacking the royal cells, and then the common ones beside them. Being unacquainted with fertile workers when I first observed this fact, I could not conceive from what motive the fury of the queen was thus directed towards the common cells. But now I know that they can distinguish the species included, and have the same instinctive jealousy or aversion at them as against the nymphs of queens properly so denominated.

reason of their laying few is from the imperfect evolution of their ovaries, but I can form no conclusions why all the eggs should be those of males, nor can I better account for their use in hives; and hitherto I have not made any experiments on their mode of fecundation.—*Pregny*, 25*th August* 1791.

LETTER VI.

ON THE COMBATS OF QUEENS: THE MASSACRE OF THE MALES: AND WHAT SUCCEEDS IN A HIVE WHERE A STRANGER QUEEN IS SUBSTITUTED FOR THE NATURAL ONE.

M. de Reaumur had not witnessed every thing relative to bees when he composed his history of these industrious animals. Several observers, and those of Lusatia in particular, have discovered many important facts that escaped him; and I, in my turn, have made various observations on some, of which he had no suspicion; at the same time—and this is a very remarkable circumstance—not only all that he expressly declares he saw has been verified by succeeding naturalists, but all his conjectures are found just. The German naturalists, Schirach, Hattorf, and Riem, sometimes contradict him, indeed, in their memoirs, but I can maintain that, while combating the opinions of M. de Reaumur, it is they who are almost always wrong; of which several instances might be adduced.

What I shall now proceed to say will give me an opportunity of detailing some interesting facts.

M. de Reaumur observed, that when any supernumerary queen was either produced in a hive, or came into it, one of the two soon perished. He has not actually witnessed the combat in which she falls, but he conjectures there is a mutual attack, and that the empire remains with the strongest or the most fortunate. Schirach, on the other hand, and after him Riem, thinks that the working bees assail the stranger, and sting her to death. I cannot comprehend by what means they were able to make this observation. Having used none except very thick hives, containing several rows of combs, they could at best see nothing but the commencement of hostilities. While the combat lasts, the bees move with great rapidity, they fly on all sides, and, gliding between the combs, conceal their motions from the observer. For my part, though using the most favourable hives, I have never seen a combat between the queen and the workers, but I have very often beheld one between the queens themselves.

In one of my hives in particular, there were five or six royal cells, each including a nymph. The eldest first underwent its transformation. Scarcely did ten minutes elapse from the time of her leaving her cradle, when she visited the other royal cells still close. Furiously attacking the nearest, by dint of labour she succeeded in opening the top, and we saw her tearing the silk of the cocoon with her teeth, but probably her efforts were inadequate to the object, for she abandoned this end of the cell, and began at the other, where she effected a larger aperture. When it was of sufficient size, she endeavoured to introduce her belly, and made many exertions, until she succeeded in giving her rival a deadly wound with her sting. Then quitting the cell, all the bees that hitherto had been spectators of her labour began to enlarge the opening, and drew out the dead body of a queen scarcely come from its envelope of a nymph.

Meanwhile, the victorious young queen attacked another royal cell, but did not endeavour to introduce her extremity into it. Here there was only a royal nymph, and no queen, come to maturity, as in the first cell. In all probability, the nymphs of queens inspire their rivals with less animosity. Still they do not escape destruction, because, whenever a royal cell has been opened before the proper time, the bees extract the contents in whatever form they may be, whether worm, nymph, or queen: and when this victorious female had abandoned the second cell, the workers, enlarging the opening which she had effected, extracted the included nymph. Lastly, the young queen attacked the third cell, but could not succeed in penetrating it. She laboured languidly, appearing as if exhausted by her previous exertions. As we now required queens for some particular experiments, we resolved to remove the other royal cells, yet in safety, to secure them from her resentment.

After these incidents, we wished to see what ensued on two queens leaving their cells at the same time, and in what manner one of them perished. I find an observation on this head in my Journal, 15th May 1790:—

Two queens quitted their cells in one of our thinnest hives almost at the same moment, and, immediately on perceiving each other, rushed together, apparently with great fury. Their position was such, that the antennæ of each were seized by the other's teeth, while the head, breast, and belly of both were mutually opposed. The extremity of their bodies had only to be curved, that they might be reciprocally pierced with the stings, and fall dead together. However it seems as if Nature has not ordained that both combatants shall perish in the duel; but rather that, when finding themselves in the situation now described, namely, opposite, and belly to belly, they shall recede at that instant with the utmost precipitation. Thus, when these two rivals felt the extremities about to meet, they disengaged themselves, and each fled away. You will observe, Sir, that I have repeated this observation very often, so that it leaves no room for doubt; and I think that here we may penetrate the intention of Nature.

There ought to be none but one queen in a hive; therefore it is necessary, if by chance a second be either produced or comes into it, that one of the two must be destroyed. This cannot be committed to the working bees, because, in a republic composed of so many individuals, a simultaneous consent cannot be supposed always to exist; it might happen frequently that one group of bees destroyed one of the females, while a second would massacre the other, and the hive thus be deprived of queens. It becomes necessary, then, that they themselves should be entrusted with the destruction of their rivals: but as, in these combats, Nature demands only a single victim, she has wisely arranged that, at the moment when, from their position, the two combatants might lose their lives, both feel so great an alarm that they think only of flight, and not of using their stings.

I am well aware of the hazard of error in minute researches into the causes of the most trifling facts; but here the object and the means seem so plain, that I have ventured to advance my conjectures. You will judge better than I can whether they are well founded. Let me now return from this digression.

A few minutes after the two queens separated, their terror abated, and each again began to seek her rival. Immediately on coming in sight, they rushed together, seized one another, and resumed exactly their former position. The result of this rencontre was the same as before. When their bellies approached, they hastily disengaged themselves, and fled with precipitation. During all this time, the workers seemed in great agitation, and the tumult appeared to increase when the adversaries separated. Two different times we observed them stop the flight of the queens, seize their limbs, and retain them prisoners above a minute. At last, that queen which was either the strongest or the most enraged, darted on her rival at a moment when unperceived, and with her teeth caught the origin of the wing; then rising above her, she brought the extremity of her own body under the belly of the other, and, by this means, easily pierced her with the sting. Quitting hold of the wing, her sting was withdrawn—the vanquished queen fell down, dragged herself languidly along, and, her strength failing, she soon expired.

This observation proved that virgin queens engage in single combats; but we wished to discover whether those that were fecundated, and mothers, entertained the same animosity.

On the 22d of July we selected a flat hive, containing a very fertile queen; and, being curious to learn whether, as virgin queens, she would destroy the royal cells, three were introduced into the middle of the comb. Whenever she became sensible of it, she sprung forward on the whole, and pierced them towards the bottom; nor did she desist until the included nymphs were exposed. The workers, who hitherto had been spectators of this destruction, now came to carry the nymphs away; they greedily devoured the food remaining at the bottom of the cells, and also sucked the fluid from the abdomen of the nymphs, and terminated with destroying the cells from which they had been extracted.

In the next place, we introduced a very fertile queen into the same hive, after painting the thorax to distinguish her from the reigning queen. A circle of bees quickly formed around the stranger, but their intention was not to caress and receive her well; for they insensibly accumulated so much, and surrounded her so closely, that in scarcely a minute she lost her liberty and became a prisoner. It is a remarkable circumstance, that other workers at the same time collected around the reigning queen and restrained all her motions—we instantly saw her confined like the stranger. Perhaps it may be said, that the bees anticipated the combat in which these queens were about to engage, and were impatient to behold the issue of it, for they retained their prisoners only when they appeared to withdraw from each other; and if one less restrained seemed desirous of approaching her rival, all the bees forming the clusters gave way to allow her full liberty for the attack; then, if the queens testified a disposition to fly, they returned to inclose them.

We have repeatedly witnessed this fact, but it presents so new and singular a characteristic in the policy of bees, that it must be seen again a thousand times before any positive assertion can be made on the subject. I would therefore recommend that naturalists should attentively examine the combats of queens, and particularly ascertain what part is taken by the workers. Is their object to accelerate the combat? Do they by any secret means excite the fury of the combatants? Whence does it happen that, accustomed to bestow every care on their queen in certain circumstances, they oppose her preparations to avoid impending danger?

A long series of observations is necessary to solve these problems. It is an immense field for experiment, which will afford infinitely curious results. I entreat you to pardon my frequent digressions. The subject is deeply philosophical: genius such as yours is requisite to treat it properly; and I shall now be satisfied with proceeding in the description of the combat.

The cluster of bees that surrounded the reigning queen having allowed her some freedom, she seemed to advance towards that part of the comb where her rival stood; then all receded before her, the multitude of workers separating the two adversaries gradually dispersed, until only two remained; these also removed, and allowed the queens to come in sight. At this moment the reigning queen rushed on the stranger, with her teeth seized her near the origin of the wing, and succeeded in fixing her against the comb without any possibility of motion or resistance. Next curving her body, she pierced this unhappy victim of our curiosity with a mortal wound.

In the last place, to exhaust every combination, we had still to examine whether a combat would ensue between two queens, one impregnated and the other a virgin ; and what circumstances attended it.

On the 18th of September we introduced a very fruitful queen into a glass hive, already containing a virgin queen, putting her on the opposite side of the comb, that we might have time to see how the workers would receive her. She was immediately surrounded, but confined only a moment. Being oppressed with the necessity of laying, she dropped some eggs, however, we could not discover what became of them—certainly the bees did not convey them to the cells, for, on inspection, we found none there. The group surrounding this queen having dispersed a little, she advanced towards the edge of the comb, and soon approached very near the virgin queen. When in sight, they rushed together, the virgin queen got on the back of the other and gave her several stings in the belly, but, being aimed at the scaly part, they did her no injury, and the combatants separated. In a few minutes they returned to the charge, and this time the impregnated queen mounted on her rival, however, she sought in vain to pierce her, for the sting did not enter ; the virgin queen then disengaged herself and fled, and she also succeeded in escaping another attack where her adversary had the advantage of position. These rivals appeared nearly of equal strength, and it was difficult to foresee to which side victory would incline, until at last, by a successful exertion, the virgin queen mortally wounded the stranger, and she expired in a moment. The sting had penetrated so far that the victor was unable to retract it, and she was overthrown by the fall of her enemy. She made great exertions to disengage the sting, but could succeed by no other means than turning on the extremity of her belly, as on a pivot. Probably its barbs fell by this motion, and, closing spirally around the stem, came more easily from the wound.

These observations, Sir, I think, will satisfy you respecting the conjecture of our celebrated Reaumur. It is certain that if several queens are introduced into a hive, one alone will preserve the empire; that the others will perish from her attacks; and that the workers at no time will attempt to employ their stings against a stranger queen. I can conceive what has misled Riem and Schirach; but it is necessary, in explaining it, that I should relate a new feature in the policy of bees at considerable length.

In the natural state of hives, several queens, issuing from different royal cells, may sometimes exist at the same moment, and they will remain either until formation of a swarm or a combat among them decides to which the throne shall appertain. But excepting this case, there never can be supernumerary queens, and if an observer wishes to introduce one, he can accomplish it only by force, that is, by opening the hive. In a word, no queen can insinuate herself into a hive in the natural state, for the following reasons:—

Bees preserve a sufficient guard, day and night, at the entrance of their habitation. These vigilant centinels examine whatever is presented, and, as if distrusting their eyes, they touch with the antennæ every individual endeavouring to penetrate the hive, and also the various substances put within their reach, which affords us an opportunity of observing that the antennæ are certainly the organs of feeling. If a stranger queen appears, her entry is prevented by the bees on guard instantly laying hold of her legs or wings with their teeth, and crowding so closely around her, that she cannot move. Other bees, from the interior of the hive, gradually come to their assistance, and confine her still more narrowly, all keeping their heads towards the centre where the queen is inclosed; and they remain with such evident anxiety, eagerness, and attention, that the cluster formed by them may be carried about for some time without their being sensible of it. A stranger queen, so closely confined and hemmed in, cannot possibly penetrate the hive. If the bees retain her too long imprisoned, she perishes, her death probably ensuing from hunger, or the privation of air. It is undoubted, at least, that she never is stung. Except in a single instance, we never have seen the bees direct their stings against her, and then it was owing to ourselves. Compassionating a queen's situation, we endeavoured to remove her from the centre of a cluster—the bees became enraged, and, in darting out their stings, some struck the queen and killed her. It is so certain that the stings were not purposely directed against her that several of the workers were themselves killed; and surely they could not intend destroying one another. Had not we interfered, they would have been content with confining the queen, and she would not have perished.

It was in similar circumstances that Riem saw the workers anxiously pursue a queen. Believing that they designed to sting her, he concluded that the office of the common bees is to kill supernumerary queens. You have quoted his observations in the *Contemplation de la Nature, part* 11, *chap.* 27, *note* 7. But you are sensible, Sir, from these details, that he has been mistaken. He did not know the attention paid by bees to what passes at the entrance of their hive, and he was entirely ignorant of the means they take to prevent the access of supernumerary queens.

After ascertaining that workers in no situation sting such supernumerary females, we were curious to learn how a stranger queen would be received in a hive deprived of the reigning one. To elucidate this matter we made numerous experiments, the detail of which would protract this letter too much, therefore, I shall relate only the principal results.

Bees are not immediately aware of the removal of their queen; their labours are uninterrupted; they watch over the young, and perform the whole of their ordinary occupations. But, in a few hours, agitation ensues—all appears a scene of tumult in the hive. A singular humming is heard; the bees desert their young and rush over the surface of the combs with delirious impetuosity. Then they discover that their queen is no longer among them. But how do they ascertain it? How do the bees on the surface of one comb discover that the queen is or is not on the next comb? In treating of another characteristic of these insects, you have yourself, Sir, proposed the same question. I am incapable of answering it indeed; but I have collected some facts, that may perhaps facilitate elucidation of the mystery.

I cannot doubt that the agitation arises from the workers having lost their queen, for, on restoring her, tranquillity is instantly regained among them; and, what is very singular, they *recognise* her—you must interpret this expression strictly. Substitution of another queen is not attended with the same effect, if she is introduced into the hive within the first twelve hours after removal of the reigning one. Here the agitation continues, and the bees treat the stranger just as they do when the presence of their own leaves them nothing to desire. They surround, seize, and keep her a very long time captive, in an impenetrable cluster, and she commonly dies either from hunger or privation of air.

If eighteen hours elapse before substitution of a stranger queen for the native one removed, she is treated at first in the same manner, but the bees leave her sooner, nor is the surrounding cluster so close; they gradually disperse, and the queen is at last liberated. She moves languidly, and sometimes expires in a few minutes. However, some queens have escaped in good health from an imprisonment of seventeen hours, and ended with reigning in the hives where they had been originally ill received.

If, before substituting the stranger queen, twenty-four hours elapse, she will be well received, and reign from the moment of her introduction into the hive. Here I speak of the good reception given to a queen after an interregnum of twenty-four hours. But as this word reception is very indefinite, it is proper to enter into some detail to explain the precise sense in which I use it.

On the 15th of August I introduced a fertile queen, eleven months old, into a glass hive. The bees had been twenty-four hours deprived of their queen, and had already begun the construction of twelve royal cells, such as described in the preceding chapter. Immediately on placing this female stranger on the comb, the workers near her touched her with their antennæ, and, passing their trunks over every part of her body, they gave her honey. Then these gave place to others, that treated her exactly in the same manner. All vibrated their wings at once, and ranged themselves in a circle around their sovereign. Hence resulted a kind of agitation, which gradually communicated to the workers situated on the same surface of the comb, and induced them to come and reconnoitre in their turn what was going on. They soon arrived, and, having broke through the circle formed by the first, approached the queen, touched her with the antennæ, and gave her honey. After this little ceremony they retired, and, placing themselves behind the others, enlarged the circle, where they vibrated their wings, and buzzed without tumult or disorder, and as if experiencing some very agreeable sensation. The queen had not yet left the spot where I had put her, but in a quarter of an hour she began to move; when the bees, far from offering any opposition, opened the circle at that part to which she turned, followed her, and surrounded her with a guard. She was oppressed with the necessity of laying, and dropped her eggs. Finally, after an abode of four hours, she began to deposit male eggs in the cells she met with.

While these events passed on the surface of the comb where the queen stood, all was quiet on the other side. There the workers apparently were unconscious of a queen's arrival in the hive. They laboured with great activity at the royal cells, as if ignorant that they no longer stood in need of them; they watched over the royal worms, supplied them with jelly and the like. But the queen having at length come hither, she was received with the same respect that she had experienced from their companions on the other side of the comb. They encompassed her, gave her honey, and touched her with their antennæ; and what proved better that they treated her as a mother, was their immediately desisting from work at the royal cells; they removed the worms, and devoured the food collected around them. From this moment the queen was recognised by all her people, and conducted herself in this new habitation as if it had been her native hive.

These particulars will give a just idea of the manner in which bees receive a stranger queen. When they have had time to forget their own, she is treated exactly as if she were their natural one, except that there is perhaps at first greater interest testified in her, or more conspicuous demonstrations of it. I am conscious of the impropriety of these expressions, but M. de Reaumur in some degree authorises them. He does not scruple to say, that bees pay *attention, homage,* and *respect,* to their queen, and, from his example, similar words have escaped most authors who treat on bees.

Twenty-four or thirty hours' absence is sufficient to make them forget their first queen; but I can hazard no conjecture on the cause.

Before terminating this letter, which is full of combats and disastrous scenes, perhaps I should give you an account of some more pleasing and interesting facts relative to their industry. However, to avoid returning to duels and massacres, I shall here subjoin my observations on the destruction of the males.

You will remember, Sir, it is agreed by all observers, that, at a certain period of the year, the workers expel and kill the drones. M. de Reaumur speaks of these executions as a horrible massacre. He does not expressly affirm, indeed, that he has himself witnessed it, but what we have seen corresponds so well with his account, that there can be no doubt he has beheld its peculiarities.

It is usually in the months of July and August that bees free themselves of the males. Then they are drove away and pursued to the inmost parts of the hive, where they collect in numbers; and as at the sametime we have found many dead on the ground before the hives, it appeared indubitable that, after being expelled, the bees stung them to death. Yet, on the surface of the comb, we do not see the sting used against them, for the bees are content to pursue and drive them away. You remark this, Sir, yourself, in the new notes added to the *Contemplation de la Nature*; and you seem disposed to think that the drones, forced to retire to the extremity of the hive, perish from hunger. Your conjectures were extremely probable. Still it was possible that the carnage might have taken place in the bottom of the hive, and had been unobserved, because that part is dark, and escapes the observer's eye.

To appreciate the justice of this suspicion, we thought of making the support of the hive of glass, and of placing ourselves below to see what passed in the scene of action. Therefore, a glass table was constructed, on which were put six hives with swarms of the same year; and, lying under it, we endeavoured to discover how the drones were destroyed. This contrivance succeeded to admiration. On the 4th of July, we saw the workers actually massacre the males, in the whole six swarms, at the same hour, and with the same peculiarities. The glass table was covered with bees full of animation, rushing upon the drones as they came from the bottom of the hive; they seized them by the antennæ, the limbs, and the wings, and, after having dragged them about, or, so to speak, after quartering them, they killed them by repeated stings directed between the rings of the belly. The moment that this formidable weapon reached them was the last of their existence—they stretched their wings and expired. At the same time, as if the workers did not consider them as dead as they appeared to us, they still darted it so deep that it could hardly be withdrawn, and they were obliged to turn round upon themselves before the stings could be disengaged.

Next day, having resumed our former position, we witnessed new scenes of carnage. During three hours, the bees furiously destroyed the males. They had massacred all their own on the preceding evening, but now they attacked those which, driven from the neighbouring hives, had taken refuge amongst them. We saw them also tear some remaining nymphs of this species from the combs; they greedily sucked all the fluid from the abdomen, and then carried them away. The following day no drones remained in the hives.

These two observations seem to me decisive. It is incontestible that Nature has charged the workers with destruction of the males at certain seasons of the year; but what means does she use to excite their fury against them? This also is one of those questions that I cannot pretend to answer. However, an observation I have made may one day lead to solution of the problem. The males are never destroyed in hives deprived of queens; on the contrary, while a savage massacre prevails in other places, they here find an asylum. They are tolerated and fed, and many are seen even in the middle of January. They are also preserved in hives which, without a queen properly so called, have some individuals of that species that lay the eggs of males, and in those whose half-fecundated queens, if I may use the expression, propagate only drones. Therefore the massacre takes place in none but hives where the queens are completely fertile, and it never begins until the season of swarming is past.*—*Pregny*, 28*th August* 1791.

* Several interesting observations, by an intelligent and impartial Scottish naturalist, the Rev. Mr Dunbar, have recently appeared, relative to the combats of queens, and some other topics in the history of bees.—*T.*

LETTER VII.

SEQUEL OF EXPERIMENTS ON THE RECEPTION OF A STRANGER QUEEN—M. DE REAUMUR'S OBSERVATIONS ON THE SUBJECT.

I HAVE frequently testified my admiration of M. de Reaumur's observations on bees. I feel sensible pleasure in acknowledging, that, if I have truly made any progress in the art of observation, I am indebted for it to profound study of the works of this excellent naturalist. In general his authority has such weight, that I can scarcely trust my own experiments when the results are different from those obtained by him. Likewise, on finding myself in opposition to the *historian of bees*, I repeat my experiments; I vary the mode of conducting them; I examine with the utmost caution all the circumstances that mislead me; and never are my labours interrupted before acquiring the moral certainty of avoiding error. With the aid of these precautions, I have discovered the justice of M. de. Reaumur's suggestions; and I have a thousand times seen, that if certain experiments seemed to combat them, it was from incorrectness of execution. Yet I must except some cases, where my results have been constantly different from his. Those respecting the reception of a stranger queen substituted for the natural one are of the number.

If, after removing the natural queen, a stranger is immediately substituted, the usurper is ill received. I never could succeed in making the bees adopt her, but by allowing an interval of twenty or twenty-four hours to elapse. Then they appeared to have forgot their own queen, and respectfully received any female put in her place. M. de Reaumur, on the contrary, affirms, that, should the original queen be removed, and another introduced, this new one will be perfectly well received from the beginning. As evidence of his assertion, he gives the detail of an experiment which ought to be read in his work, for here I shall quote only an extract of it.* He induced four or five hundred bees to leave their native hive and enter a glass box, containing a small piece of comb towards the top. At first they were in great agitation, and, to pacify or console them, he presented a new queen. From that moment the tumult ceased, and the stranger queen was received with all respect.

I do not dispute the truth of this experiment, but, in my opinion, it does not warrant the conclusion deduced by M. de Reaumur. His apparatus removed the bees too much from their natural condition, to allow him to judge of their instinct and dispositions. In other situations, he has himself observed these animals reduced to small numbers, lose their industry and activity, and feebly continue their ordinary labours. Thus their instinct is affected by every operation that

* Edit. 4to, tom. v. p. 258.

diminishes their number too much.* To render such an experiment truly conclusive, it must be made in a populous hive; and on removing the native queen, the stranger must be immediately substituted in her place. Had this been done, I am quite persuaded that M. de Reaumur would have seen the bees imprison the usurper, confine her at least twelve or fifteen hours among them, and frequently suffocate her; nor would he have witnessed any favourable reception before an interval of twenty-four hours after removal of the original queen. No variation has occurred in my experiments regarding this fact. Their number, and the attention bestowed on them, make me presume that they merit your confidence.

M. de Reaumur, in another passage of the same Memoir, affirms that *bees, having a queen they are satisfied with, are nevertheless disposed to give the best possible reception to any female seeking refuge among them.* In the preceding letter, I have related my experiments on this head—their issue has been very different from that of M. de Reaumur. I have proved that the workers never employ their stings against the queen, but this cannot be called welcome reception of a stranger. They retain her within their ranks, and seem to allow her liberty only when she prepares to combat the reigning queen. This observation cannot be made except in the thinnest hives. Those used by M. de Reaumur always had two parallel combs at least, which must have prevented him from

* The author elsewhere observes, that they even cease to keep a guard at the entrance of the hives in such situations. —*T.*

remarking some very important circumstances that influence the conduct of workers when supplied with several females. The first circles formed around a stranger queen he has taken for caresses, and, from the little that his queen could advance in view between the combs, it must have been impossible for him to observe that the circles, which were always gradually contracting, ended in restraint of the females there inclosed. Had he used thinner hives, he would have discovered that what he supposed an indication of favourable reception was the prelude of actual imprisonment.

I am reluctant to assert that M. de Reaumur was deceived; yet I cannot admit that, on certain occasions, bees tolerate a plurality of females in their hives; neither will the experiment on which this affirmation rests be considered decisive. In the month of December, he introduced a stranger queen into a glass hive in his cabinet, and confined her there, while the bees had not an opportunity of going out. The stranger was well received; her presence awakened the workers from their lethargic state, into which they did not relapse; she excited no carnage; the number of dead bees on the board of the hive did not sensibly increase, and no dead queens were found.

Before concluding any thing favourable to the plurality of queens, it was necessary to ascertain whether the native female was living when the new one was introduced into the hive. However, the author neglected it, and, it is very probable that the hive had lost its queen, since the bees were languid, and the presence of a stranger restored their activity.

I trust, Sir, that you will pardon this slight criticism. Far from industriously seeking faults in our celebrated Reaumur, I derive the greatest pleasure when my observations coincide with his, and more especially when my experiments justify his conjectures, but I think it proper to point out those cases where the imperfections of his hives have led him into error, and to explain from what causes I have not seen certain facts under the same aspect he did. I feel particular anxiety to merit your confidence, and I am aware that the greatest exertions are necessary, when I have to combat the historian of bees. I confide in your judgment, and pray you to be assured of my respect.—*Pregny, 30th August* 1791.

LETTER VIII.

IS THE QUEEN OVIPAROUS?—WHAT INFLUENCE HAS THE SIZE OF THE CELLS, WHEREIN THE EGGS ARE DEPOSITED, ON THE BEES PRODUCED?—RESEARCHES ON THE MODE OF SPINNING THE COCOONS.

In this letter I shall collect together some isolated observations relative to various points in the history of bees, concerning which you wished me to engage.

You were desirous that I should investigate whether the queen be really oviparous—a question left undecided by M. de Reaumur, who says he never saw the worm hatched, and is content with remarking, that worms are found in cells where eggs have been deposited three days previously. If we resolve to attempt to seize the moment of the worm quitting the egg, it is necessary that our inquiries be carried beyond the interior of the hive, for there the continual motion of the bees obscures what passes at the bottom of the cells. The egg must be taken out, presented to the microscope, and every change attentively watched. One other precaution is essential—as a certain degree of heat is requisite for hatching the worms, should the eggs be too soon deprived of it they wither and perish—therefore, the sole method to succeed in observing the worm come out, consists in watching the queen while she lays, in marking the egg so as to be recognised, and in removing it from the hive to the microscope only an hour or two before the three days elapse. The worm certainly will be hatched provided it has been exposed as long as possible to the full degree of heat. Such is the course I have pursued, and the following are the results obtained:—

In the month of August, we removed several cells containing eggs that had been three days deposited, cut off the top of the cells, and put the pyramidal bottom, where the egg was fixed, on a glass slider. Slight motions were soon perceptible in one of the eggs. At first, we could not discern any external organization—the worm was entirely concealed from us by its pellicle, and we then prepared to examine the egg with a powerful magnifier. However, during the interval, it burst its surrounding membrane, and cast off part of the envelope, which was torn and ragged on different parts of the body, and more evidently so towards the last rings. It curved and stretched itself alternately with very lively action, and occupied twenty minutes in casting off the exuviæ; when ceasing its exertions, it lay down, bent the body, and seemed to take that rest which it now required. An egg laid in a worker's cell produced this insect, which would have become a worker itself.

We next directed our attention to the moment when a male worm should be hatched. An egg was exposed to the sun on a glass slider, and, with a good magnifier, nine rings of the worm were perceptible within the transparent pellicle. This membrane was still entire, and the worm perfectly motionless. The two longitudinal lines of trachæ were visible on the surface, with many ramifications proceeding from them. We never lost sight of the egg a single instant, and now succeeded in observing the first motions of the worm. The thick end alternately straightened and curved, and extended almost to the part where the sharp extremity was fixed. These exertions burst the membrane, first on the upper part towards the head, then on the back, and afterwards on all the rest of the body successively—the ragged pellicle remained in folds on different places of it, and then fell off. Thus it is beyond dispute, that the queen is oviparous.

Some observers affirm, that the workers watch on the eggs before the worms are hatched; and it is certain that, at whatever time the hive is examined, we always see them with the head and thorax inserted into cells containing eggs, and remaining motionless several minutes in that position. It is impossible to discover what they do, for the interior of the cell is concealed by their bodies; but it is very easily ascertained that, in this attitude, they are doing nothing to the eggs.

If, at the moment the queen lays, her eggs are put into a grated box, and deposited in a strange hive, where there is the necessary degree of heat, the worms come out at the usual time, just as if they had been left in the cells. Thus, no extraordinary aid or attention is required for promoting their exclusion.

When the workers penetrate the cells, and remain fifteen or twenty minutes motionless, I have reason to believe that it is only to repose from their labours. My observations on this subject seem correct. You know, Sir, that some irregular shaped cells are frequently constructed on the panes of the hive. These, being glass on one side, are exceedingly convenient to the observer, since all that passes within is exposed. I have often seen bees enter such cells when nothing could attract them. They contained neither eggs nor honey, nor did they need farther completion, therefore, the workers repaired thither only to enjoy some repose. Indeed, they were fifteen or twenty minutes so perfectly motionless, that had not the dilatation of the rings showed their respiration, we might have concluded them dead. The queen also sometimes penetrates the large cells of the males, and continues very long motionless. Her position prevents the bees from paying their full homage to her, yet even then they do not fail to surround her in a circle, and brush the part of her belly that remains uncovered.

Drones do not enter the cells while reposing, but cluster together on the combs, and sometimes remain in this position eighteen or twenty hours without the slightest motion.

As it is important, in many experiments, to know the exact time that the three species of bees exist before assuming their ultimate form, I shall here subjoin my own observations on the point :—

The worm of workers passes three days in the egg, five in the vermicular state, and then the bees close up its cell with a wax covering. It now begins spinning its cocoon, in which operation thirty-six hours are consumed. In three days it changes to a nymph, and passes six in this form. It is only on the twentieth day of its existence, counting from the moment the egg is laid, that the winged state is attained.

The royal worm also passes three days in the egg, and is five a worm, when the bees close its cell; and it immediately begins spinning the cocoon, which occupies twenty-four hours. The tenth and eleventh day it remains in complete repose, and even sixteen hours of the twelfth. Then the transformation to a nymph takes place, in which state four days and the third part of a fifth are passed. Thus, it is not before the sixteenth day that the perfect state of queen is attained.

The male passes three days in the egg, six and a half as a worm, and metamorphoses into a winged animal on the twenty-fourth day after the egg is laid.

Though the larvæ of bees are apodal, they are not condemned to remain absolutely motionless in their cells, for they can move in a spiral direction. During the first three days, this motion is so slow as scarcely to be perceptible, but it afterwards becomes more evident. I have then observed them perform two complete revolutions in an hour and three quarters. When the period of transformation arrives, they are only two lines from the orifice of the cells. As their position is constantly the same, bent in an arc, it follows that the larvæ in the cells of workers and drones are perpendicular to the horizon, while those in the royal cells lie horizontally. It might be thought, that the difference of position has much influence on the increment of the various larvæ, yet it has none. By reversing combs containing common cells full of brood, I have brought the worms into a horizontal position, but they were not injured. I have also turned the royal cells, so that the worms came into a vertical direction, nevertheless, their increment was neither slower nor less perfect.

The mode of bees spinning their cocoons has given me great surprise, and there I have witnessed many new and interesting facts. The worms both of workers and males fabricate *complete* cocoons in their cells, that is, close at both ends, and surrounding the whole body. On the other hand, the royal larvæ spin imperfect cocoons, open behind, and enveloping only the head, thorax, and first ring of the abdomen. The discovery of this difference, which at first may seem trifling, has been the source of extreme pleasure to me, for it evidently demonstrates the admirable art with which Nature connects the various characteristics of the industrious bees.

You recollect, Sir, the evidence that I gave you of the mutual aversion of queens, of the combats in which they engage, and the animosity that leads them to destroy one another. Of several royal nymphs in a hive, the first transformed attacks the rest, and stings them to death. But were these nymphs enveloped in complete cocoons, she could not accomplish it. Why? because the silk is of so close a texture, the sting could not penetrate, or if it did, the barbs would be retained by the meshes of the cocoon, and the queen, unable to retract it, would become the victim of her own fury. Thus, that she may destroy her rivals, it is necessary the last rings of the body shall remain uncovered; and the royal nymphs must therefore form only imperfect cocoons. You will observe that the last rings alone should be exposed, for the sting can penetrate no other part: the head and thorax are protected by connected shelly plates which it cannot pierce.

Hitherto philosophers have claimed our admiration of Nature for her care in preserving and multiplying the species. But from the facts I relate, we must now admire her precautions in exposing certain individuals to a mortal hazard.

The detail on which I have just entered clearly indicates the final cause of the opening left by the royal worms in their cocoons; but it does not show whether they do so in consequence of a particular instinct, or whether the wideness of their cells prevents them from stretching the thread up to the top. This question interested me very much, and the only method of deciding it was to observe the worms while spinning, which could not be done from their cells being opaque. It then occurred to me to dislodge them from their own habitations, and introduce them into glass tubes, blown in exact imitation of the different kind of cells. The most difficult part of the operation consisted in extracting the worms and introducing them here, but my assistant accomplished it with much address. He opened several sealed royal cells, where we knew the larvæ were about to begin their cocoons, and, taking them gently out, introduced one into each of my glass cells without the smallest injury.

They soon prepared to work, and commenced by stretching the anterior part of the body in a straight line, while the other was bent in a curve, thus forming an arc of which the longitudinal sides of the cells were tangents, and afforded two points of support. The head was next conducted to the different parts of the cell which it could reach, and carpeted the surface with a thick bed of silk. We remarked that the threads were not carried from one side to another, which would have been impracticable, for the worms, being obliged to support themselves, had to keep the posterior rings curved, and, the free and moveable part of the body was not long enough to admit of the mouth reaching the sides diametrically opposite to fix the threads to them. You will remember, Sir, that the royal cells are of a pyramidal form, with a wide base, and a long diminished top. They hang perpendicularly in the hive, the point downwards, from which position the royal worm can be sustained in the cell, only when the curvature of the posterior part gains two points of support, and it cannot obtain this support without resting on the lower part, or towards the extremity. Therefore, if attempting to stretch out and spin towards the wide end of the cell, it could not reach both sides from being too distant. One part would be touched by its extremity, and the other by its back, consequently it would tumble down. I have particularly ascertained the fact in glass cells that were too large, and of which the diameter was greater towards the point than is usual in cells; there they were unable to sustain themselves.

These first experiments obviated the probability of any particular instinct in the royal worms. They proved, that if spinning incomplete cocoons, it was because they were forced to do so by the figure of their cells. However, wishing to have evidence still more direct, I put them into cylindrical glass cells, or portions of glass tubes resembling common cells, where I had the satisfaction of seeing them spin complete cocoons, as the worms of workers do. Lastly, I put common worms into very wide cells, and they left the cocoon open. It is demonstrated, therefore, that the royal worms, and those of workers, have the same instinct and the same industry, or in other words, when situated in similar circumstances, the course they follow is the same. I may here add, that the royal worms artificially lodged in cells, where they can spin complete cocoons, undergo all their metamorphoses equally well. Thus the necessity which Nature imposes on them of leaving the cocoon open, is not on account of their increment, nor has it any other object than that of exposing them to the certainty of perishing by the wounds of their natural enemy, an observation new and truly singular.

I should relate my experiments on the influence that the size of the cells has on bees—it is to you, Sir, that I am indebted for suggesting them:—

As we sometimes found males smaller than they ought to be, and also queens more diminutive than usual, it was desirable to obtain a general explanation to what degree the cells, where bees pass the first period of their existence, have an influence on their size. With this view, you advised me to remove all the combs composed of common cells, and to leave those consisting of large cells only. It was evident if the common eggs which the queen would lay in these large cells produced workers of larger size, we were bound to conclude that the size of the cells had a decided influence on the size of the bees. The first time I made the experiment it did not succeed, because weevils having lodged in the hive discouraged the bees. But I repeated it afterwards, and the result was very remarkable.

I removed the whole comb, consisting of common cells, from one of my best glass hives, and left that composed of the cells of males alone, and, to avoid vacuities, I supplied others of the same kind. This was in June, the season most favourable to bees. I expected that the bees would have quickly repaired the ravages produced by the operation in their dwelling—that they would labour at the breaches, and unite the new combs to the old, but I was very much surprised to see that they did not begin to work. Expecting the resumption of their activity, I continued observing them several days; however, my hopes were disappointed. Their homage to the queen was not interrupted indeed; but except in this, their conduct to her was quite different from what it is usually; they clustered on the combs without exciting any sensible heat. A thermometer among them rose only to 81 degrees, though standing at 77 degrees in the open air. In a word, they appeared in a state of the greatest despondency.

The queen herself, though very fertile, and though she must have been oppressed by her eggs, hesitated long before depositing them in the large cells—she chose rather to drop them at random, than they should be laid in those unsuitable. However, on the second day, we found six that had been deposited there with all regularity. The worms were hatched three days afterwards, and then we began to study their history. Notwithstanding the bees provided them with food, they did not carefully attend to it; yet I was in hopes that they might be reared. I was again disappointed, for next morning the whole worms had disappeared, and their cells remained empty. Profound silence reigned in the hive—few bees left it, and these returned without pellets on their limbs—all was cold and inanimate. To promote a little motion, I thought of supplying the hive with a comb, composed of small cells, full of male brood of every age. The bees, after twelve days obstinately abstaining from working in wax, did not unite this comb to their own. However, their industry was awakened in a way that I had not anticipated. They removed the brood from it, cleaned out the whole cells, and prepared them for receiving new eggs. I cannot determine whether they expected the queen to lay, but it is certain if they did so they were not deceived. From this moment, she no longer dropped her eggs, but laid such a number in the new comb, that we found five or six together in the same cell. I then removed all the combs composed of large cells to substitute small cells in their place, an operation which restored complete activity among the bees.

The peculiarities of this experiment seem worthy of attention. It proves that Nature does not allow the queen the choice of the eggs she is to lay. It is ordained that, at a certain time of the year, she shall produce those of males, and at another time the eggs of workers—an order which cannot be inverted. We have seen that another fact led me to the same consequence, and, as that was extremely important, I am delighted to have it confirmed by a new observation. Let me repeat, therefore, that the eggs are not indiscriminately mixed in the ovaries of the queen, but arranged so, that, at a particular season, she can lay only a certain kind. Thus it would be vain at that time of the year, when the queen should lay the eggs of workers, to attempt forcing her to lay male eggs, by filling the hives with large cells, for, by the experiment just described, we learn, that she will rather drop the workers' eggs by chance than deposit them in an unsuitable place, and that she will not lay the eggs of males. I cannot yield to the pleasure of allowing to this queen discernment or foresight, for I observe a kind of inconsistency in her conduct. If she refused to lay the eggs of workers in large cells, because Nature had instructed her that their size was neither proportioned to the size nor necessities of common worms, would not she also have been instructed to abstain from laying several eggs in one cell? It seems much easier to rear a worker's worm in a large cell, than to rear several of the same species in a small one. Therefore, the supposed discrimination of the queen bee is not very conspicuous. Here the most prominent feature of industry appears in the common bees. When supplied with a comb of small cells, full of male brood, their activity was awakened; but instead of bestowing the necessary care on this brood, as they would have done in every other situation, they destroyed the whole nymphs and larvæ, and cleaned out their cells, that the queen, now oppressed with the necessity of laying, might suffer no impediment to depositing her eggs. Could we allow them either reason or reflection, this would be an interesting proof of their affection for her.

The experiment, now detailed at length, not having fulfilled my object in determining the influence of the size of the cells on that of the worms, I devised another which proved more successful.

Having selected a comb composed of large cells, containing the eggs and worms of males, I removed all the worms from their farina, and, my assistant substituting those of workers a day old in their place, he introduced it into a hive that had the queen. The bees did not abandon these substituted worms—they covered their cells with a top almost flat, of a kind quite different from what is put on the cells of males, which shows that they were well aware that, though inhabiting large cells, they were not males. This comb remained eight days in the hive, counting from the time that the cells were sealed. I then removed it to examine the included nymphs, which proved those of workers in different stages of advancement, but, as to size and figure, they perfectly resembled what had grown in the smallest cells. Thence I concluded, that the larvæ of workers do not acquire greater size in large than in small cells. Although the experiment was made only once, it seems decisive. Nature has appropriated cells of certain dimensions for the worms of workers while in their vermicular state; undoubtedly she has ordained that their organs shall be fully expanded, and as there is sufficient space for that purpose, more would be useless. Their evolution ought to be no greater in the most spacious cells than in those appropriated for them. If some cells smaller than common ones are found in combs, and the eggs of workers are deposited there, the size of the bees probably will be less than that of common workers, because they have been cramped in the cells; but it does not ensue, that a larger cell will admit of them growing to a greater size.

The effect produced on the size of drones by the size of the cells their worms inhabit, may serve as a rule for what should happen to the larvæ of workers in the same circumstances. The large cells of males are sufficiently capacious for the full expansion of their organs. Thus, although reared in cells of still greater capacity, they will grow no larger than common drones. We have had evidence of this in those produced by queens whose fecundation has been retarded. You will remember, Sir, that they sometimes lay male eggs in the royal cells. Now, the males proceeding from these eggs, and reared in cells much more spacious than Nature has appropriated for them, are no larger than common males. Therefore it is certain, that whatever be the size of the cells where the worms acquire their increment, the bees will attain no greater size than is peculiar to their species. But if, in their primary form, they live in cells smaller than they should be, as their growth then will be checked, they will not attain the usual size, of which there is proof in the following experiment:—

I had one comb consisting of the cells of large drones, and another with those of workers, which also served for the male worms. Of these my assistant took a certain number from the smallest cells, and deposited them on a quantity of food purposely prepared in the large ones, and in return he introduced into the small cells the worms that had been hatched in the others, then committing both to the care of the workers in a hive where the queen laid the eggs of males only. The bees were not affected by this change—they took equal care of the worms, and, when the period of metamorphosis arrived, gave both kinds of cells that convex covering usually put on those of males. Eight days afterwards we removed the combs, and found, as I had expected, nymphs of large males in the large cells, and those of small males in the small ones.

On your suggestion I carefully made another experiment, but it met with an unforeseen obstacle. To appreciate the influence of the royal food on the evolution of the worms, you desired me to supply the worm of a worker in a common cell with it. Twice I have attempted this operation without success, nor do I think it can ever succeed. If bees get the charge of worms in whose cells the royal food is deposited, and if at the same time they have a queen, they soon remove the worms, and greedily devour the food. When, on the contrary, they are deprived of a queen, they change the common cells containing worms into cells of the largest kind, and then the worms will be infallibly converted to queens.

But there is another situation where we can judge of the influence of the royal food administered to worms in common cells. I have spoken at great length in my letter on the existence of fertile workers. You cannot forget, Sir, that the evolution of their sexual organs is owing to the reception of some particles of royal jelly, while in the vermicular form, but, for want of new observations, I must refer you to what is previously said on the subject.—*Pregny*, 4*th September* 1791.

LETTER IX.

ON THE FORMATION OF SWARMS.

I can add but few facts to the information which M. de Reaumur has communicated relative to swarms.

A young queen, according to this celebrated naturalist, is always or almost always at the head of a swarm, but he does not assert the fact positively, and had some doubts on the subject. "Is it certain," says he, "as we have hitherto supposed, in coincidence with all who have treated of bees, that the new colony is uniformly conducted by a young mother? May not the old mother be disgusted with her habitation? or may she not be influenced by some particular circumstances, to abandon all her possessions to the young female? I wish it had been in my power to solve this question otherwise than by mere probabilities, and that some misfortune had not befallen all the bees whose queen I had marked red on the thorax." These expressions seem to indicate, that M. de Reaumur suspected that the old queens sometimes conduct the young swarms. From the following details, you will observe that his conjectures are fully justified.

In the course of spring and summer the same hive may throw several swarms. The old queen is always at the head of the first colony, the others are conducted by young queens. Such is the fact which I shall now prove, and the peculiarities attending it shall be also related.

But previous to entering on this subject, I shall repeat what already has been frequently observed, that the *leaf* or flat hives are indispensable in studying the industry and instinct of bees. When these insects are left at liberty to construct several rows of parallel combs, we are precluded from observing what continually passes between them, or they must be dislodged by water or smoke, for the purpose of examining their structures—a violent proceeding, which has a material influence on their instinct, and consequently exposes an observer to the risk of supposing simple accidents permanent laws.

I now proceed to experiments proving that an old queen always conducts the first swarm :—

One of my glass hives consisted of three parallel combs, placed in frames opening like the leaves of a book. It was well peopled, and abundantly provided with honey and wax, and with brood of every different age. On the fifth of May 1788, I removed its queen, and, on the sixth, transferred all the bees from another hive into it, with a fertile queen at least a year old. They entered easily and without fighting, and were in general well received. The old inhabitants of the hive, which, since privation of their queen, had begun twelve royal cells, also gave the fertile queen a good reception—they presented her with honey, and surrounded her in regular circles. However, there was a little agitation in the evening, though confined to the surface of the comb where we had put the queen, and which she had not quitted, for all was perfectly quiet on the other side.

On the morning of the seventh, the bees had destroyed the twelve royal cells, but, independent of that, order continued prevalent in the hive—the queen laid the eggs of males in the large cells, and those of workers in the small ones, respectively.

Towards the twelfth, we found the bees occupied in constructing twenty-two royal cells, of the same species described by M. de Reaumur, that is, the bases not in the plane of the comb, but appended perpendicularly by pedicles or stalks of different length, like stalactites, on the edge of the passage made by the bees through their combs. They bore considerable resemblance to the cup of an acorn, the longest being only about two lines and a half in depth from the bottom to the orifice.

On the thirteenth, the queen seemed already more slender than when introduced into the hive, however, she still laid some eggs, both in common cells and in those of males. We also surprised her this day laying in a royal cell: she first dislodged the worker there employed, by pushing it away with her head, and then supported herself by the adjoining cells while depositing the egg.

On the fifteenth, the size of the queen was yet farther reduced; the bees continued their attention to the royal cells, which were all unequally advanced, some to the height of three or four lines, while others were already an inch long; thus proving that the queen had not laid in the whole at the same time.

At the moment when least expected, the hive swarmed on the nineteenth; we were warned of it by the noise in the air, and hastened to collect and put the bees into a hive purposely prepared. Though we had overlooked the facts attending the departure of this swarm, the object of the experiment was fulfilled; for, on examination of all the bees, we were convinced they had been conducted by the old queen, by her that we introduced on the sixth of the month, and which had been deprived of one of the antennæ. Observe, there was no other queen in this colony. In the hive she had left we found seven royal cells close at the top, but open at the side, and quite empty. Eleven more were sealed, and some others newly begun; no queen remained in the hive.

The new swarm next became the object of our attention; we observed it during the rest of the year, in winter and the subsequent spring; and, in April, we had the satisfaction of seeing another depart with the same queen at its head that had conducted the former one in May of the preceding year.

You will remark, Sir, that this experiment is *positive*. We put an old queen in a glass hive while laying the eggs of males; the bees received her well, and at that time began to construct royal cells; next she laid in one of them before us; and in the last place led forth the swarm.

We have repeated the same experiment several times with equal success. Thus it appears incontestible, that the old queen always conducts the first swarm, but never quits the hive before depositing eggs in the royal cells, from which other queens will proceed after her departure. These cells are prepared by the bees only while the queen lays male eggs, which is attended by a remarkable fact, namely, that after this laying terminates, her belly being considerably diminished, she can easily fly, whereas it is previously so heavy that she can hardly drag it along. Therefore it is necessary she should lay, in order to be in a state for undertaking her journey, which sometimes may be very long.

But this single condition is not enough. It is also requisite that the bees be very numerous—they should be even superabundant, and it may be said that they are aware of it, for, if the hive is thin, no royal cells are constructed when the male eggs are laid, which is done solely at the period that the queen is able to conduct a colony. This fact was proved by the following experiment on a large scale:—

On the third of May 1788, we divided each of eighteen hives, whose queens were about a year old, into two portions. Thus each portion of the hives had but half the bees that were originally there. Eighteen halves wanted queens, but the other eighteen had very fertile ones. They soon began to lay the eggs of males; but the bees being few, they did not construct royal cells, and none of the hives threw a swarm. Therefore, if the hive containing the old queen is not very populous, she remains in it until the subsequent spring, and, if the population is then sufficient, royal cells will be constructed—she will begin to lay male eggs, and, after depositing them, will issue forth at the head of a colony, before the young queens are produced.

Such is a brief abstract of my observations on swarms conducted by old queens. You must excuse the long detail on which I am about to enter, concerning the history of the royal cells left by the queen in the hive. Every thing relative to this part of the history of bees has been hitherto very obscure. A long course of observations, protracted even during several years, was necessary to remove, in some degree, the veil concealing the mystery. I have been indemnified for the trouble, indeed, by the pleasure of seeing my experiments reciprocally confirmed, but, considering the assiduity required in these researches, they were truly very laborious.

Having established in 1788 and 1789, that queens a year old conducted the first swarm, and that they left worms or nymphs in the hive, which were to transform into queens in their turn, I endeavoured, in 1790, to profit by the goodness of the spring, to study all that related to these young queens; and I shall now extract the chief experiments from my Journal:—

On the fourteenth of May, we transferred two portions of bees from their straw hives into a very thin large glass hive, and allowed them only one queen of the preceding year, which had already commenced laying in her native hive. We introduced her on the fifteenth. She was very fertile. The bees received her well, and she quickly began to lay in small and large cells alternately.

On the twentieth, we saw the fabrication of twelve royal cells, all on the edges of the communications or passages through the combs, and shaped like stalactites.

Ten were much though unequally enlarged on the twenty-seventh, but none so long as when the worms are hatched.

On the twenty-eighth, previous to which the queen had not ceased laying, her belly was very slender, and she began to exhibit signs of agitation. Her motion soon became more lively, yet she still continued examining the cells as when about to lay, sometimes introducing half her belly, but suddenly withdrawing it without having done so. At other times, inserting it no farther, she deposited an egg, which was not fixed by an end to the bottom of the cell, but rested irregularly on one side of the hexagon. The queen produced no distinct sound in her course, nor did we hear any thing different from the ordinary humming of bees. She sometimes stopped while passing over those in her way, and the bees also stopped as if to consider her. Advancing briskly, they struck her with their antennæ, and mounting on her back, she proceeded carrying several above her. None gave her honey, but she took it voluntarily in passing over the cells. She was no longer surrounded and enclosed by bees in regular circles. The first that were aroused by her motions followed her running in the same manner, and in their passage excited those still tranquil on the combs. The track of the queen was evident after she left it, by the agitation created, which was never afterwards quelled.

She had soon visited every part of the hive, and disseminated a general sensation: if some places still remained tranquil, the bees in agitation arrived, and communicated their disorder. The queen ceased to deposit her eggs in cells; she dropped them at random. Nor did the bees continue to watch over the young; they ran through the hive in every direction; even those returning from the fields before the confusion came to its height, no sooner entered their dwelling than they participated in these tumultuous motions. Neglecting to free themselves of the waxen pellets on their limbs, they hurried blindly about. At last the whole rushed precipitately towards the outlets of the hive, and the queen along with them.

As it was of much consequence to see the formation of new swarms in this hive, I wished it still to continue very populous, and on that account removed the queen at the moment she came out, that the bees might not fly too far, and likewise that they might return. In fact, after losing their female, they did return to the hive. To increase the population still more, I added another swarm, which had come from a straw hive on the same morning, and removed its queen also.

Athough these facts were clear, and apparently not susceptible of any error, I was particularly earnest to discover whether old queens always followed a similar course. Therefore, on the twenty-ninth, I replaced in her glass hive the queen, a year old, hitherto the subject of my experiments, and which had just begun to lay the eggs of males. On the same day, finding one of the royal cells, left by the preceding queen, larger than the rest, we supposed from its length that the included worm was two days old; that it was hatched on the twenty-seventh, and that the egg had been laid on the twenty-fourth. On the thirtieth the queen laid a great deal in the large and small cells alternately. Then and on the two following days, the bees enlarged several royal cells, but unequally, which proved that they included larvæ of different ages. One was closed on the first of June, another on the second, and some new ones were also commenced. All was perfectly quiet at eleven in the morning, but, at mid-day, the queen, from the utmost tranquillity, became suddenly agitated; and her disorder insensibly communicated to the workers in every part of their dwelling. In a few minutes they precipitately crowded to the outlets, and along with the queen left the hive. After they had settled on the branch of a neighbouring tree, I sought for the queen, thinking that, by removing her, the bees might return, which actually ensued. Their first care seemed devoted to seeking the female; they were still in great agitation, but gradually calmed, and in three hours complete tranquillity was restored.

On the third they had resumed their usual occupations—they attended to the young, worked within the open royal cells, and also watched on those that were shut. They made a waved work on them, which is not done by applying wax bands, but by removing wax from the surface. Towards the top this waved work is almost imperceptible; it becomes deeper above, and the workers excavate it still more from thence to the base of the pyramid. The cell, when once shut, also becomes thinner, and is so much reduced, immediately preceding the queen's metamorphosis from a nymph, that all the motions within are perceptible through the thin covering of wax on which the waved work is founded. It is a very remarkable circumstance, that in making the cells thinner, from the moment they are closed, the bees know to regulate their labour, so that it terminates only when the nymph is ready to undergo its last metamorphosis.

On the seventh day the cocoon is almost completely *unwaxed*, if I may use the expression, at the part next to the head and thorax of the queen. This operation facilitates her exit, for, she has nothing to do but to cut the silk forming the cocoon. Most probably the object here is to promote evaporation of the superabundant fluids of the nymph. I have engaged in some direct experiments to ascertain the fact, but they are yet unfinished. A third royal cell was closed by the bees on the same day, the third of June, twenty-four hours after closing the second, and the like followed with respect to other royal cells successively, during the subsequent days.

Every moment of the seventh we expected the queen to leave the royal cell, which was shut on the thirtieth of May. On the preceding evening her period of seven days had elapsed ; and the waving of her cell was so deep, that what passed within was pretty perceptible. We could discern that the silk of the cocoon was cut circularly, a line and a half from the extremity ; but the bees being unwilling that she should yet quit her cell, had soldered the covering to it with some particles of wax. What seemed most singular was, this female emitting a very distinct sound or clacking, from her prison, which became still more audible in the evening, and even consisted of several monotonous notes in rapid succession.

The same sound issued from the second royal cell on the eighth. Several bees kept guard around each of the royal cells.

The first opened on the ninth. Its young queen was lively, slender, and of a brown colour. Now, we understood why bees retain the females captive in their cells after the period for transformation has elapsed—it is, that they may be able to fly the instant they are liberated. The new queen occupied all our attention.

When she approached the other royal cells, the bees on guard pulled, bit her, and chased her away; their irritation seemed to be greatly excited against her, and she enjoyed tranquillity only when at a considerable distance from these cells. This proceeding was frequently repeated throughout the day. She twice emitted the sound, standing while doing so with her thorax against a comb, and her wings crossed on her back, in motion, but without being unfolded or farther opened. Whatever might be the cause of her assuming this attitude, the bees were affected by it—all hung down their heads, and remained motionless.

The hive presented the same appearances on the following day. Twenty-three royal cells yet remained, assiduously guarded by a great many bees. When the queen approached, the guards became agitated, crowded her on all sides, bit, and commonly drove her away; sometimes when, in these circumstances, she emitted her sound, assuming the position just described, from that moment the bees became motionless.

The queen confined in the second cell, which she had not yet left, was heard to pipe several times;* and we accidently discovered how the bees fed her. By attentive inspection, a small aperture was discernible in the end of the cocoon, which she had cut in order to escape, and which her guards had again covered with wax to retain her still longer. She alternately thrust her proboscis through the cleft, and retracted

* It is not evident whether the author means that she emitted a sound similar to that just described; but a note, commonly called piping, is heard from some organ yet undiscovered.—*T.*

unperceived by the bees, until at length, attracting their attention, one of them came to apply its own to that of the captive queen, and then gave way to others also approaching her with honey. When satisfied she retracted her trunk, and the bees once more closed up the opening with wax.

The queen this day between twelve and one became extremely agitated. The royal cells had multiplied very much; she could go no where without finding them, and on her approach she was very roughly treated. Then she fled, but to obtain no better reception. At last these things agitated the bees—they precipitately rushed through the outlets of the hive, and settled on a tree in the garden. It singularly happened that the queen was unable to follow them and herself head the swarm. She had attempted to pass between two royal cells, before they were abandoned by the bees guarding them, but she was so confined and maltreated as to be incapable of moving; we transferred her into a separate hive prepared for a particular experiment. The bees, which had clustered on a branch, soon discovered their queen was not present, and returned to the hive of their own accord. Such is an account of the second colony of this hive.

We were extremely solicitous to ascertain what would become of the other royal cells. Four of the close ones had attained complete maturity, and the queens would have left them had not the bees prevented it. They were not open either previous to the agitation of the colony, or at the moment of swarming.

None of the queens were at liberty on the eleventh.

The second should have transformed on the eighth thus she had been three days confined, a longer period than the first which had occasioned the formation of the swarm. We could not discover what caused the difference in their captivity.

On the twelfth, the queen was at last liberated, as we found her in the hive. She had been treated exactly as her predecessor; the bees allowed her to rest in quiet when distant from the royal cells, but tormented her cruelly when she approached them. We watched this queen a long time, but not aware that she would lead out a colony, we left the hive for a few hours, and returning at mid-day, were greatly surprised to find it almost totally deserted. During our absence, it had thrown a prodigious swarm, which still clustered on the branch of a neighbouring tree. We also saw with astonishment the third cell open, and its top connected to it as by a hinge. In all probability the captive queen, profiting by the confusion that preceded the swarming, had escaped. Thus there was no doubt of both queens being in the swarm. So it proved—and we removed them, that the bees might return to the hive, which they did very soon afterwards.

While we were occupied in this operation, the fourth captive queen left her prison, and the bees found her on returning. At first they were very much agitated, but calmed towards the evening, and resumed their wonted labours. They formed a strict watch around eighteen royal cells now remaining to be guarded, and took great care to repulse the queen whenever she attempted to approach them.

The fifth queen left her cell at ten at night, therefore, two queens were now in the hive. They immediately began fighting, but came to disengage themselves from each other. However, they fought several times during the night without any thing decisive. Next day, the thirteenth, we witnessed the death of one, which fell by the wounds of her enemy. This duel was quite similar to what is said of the combats of queens.

The victorious queen now presented a very singular spectacle. She approached a royal cell, and took this moment to utter the sound, and assume that posture, which strike the bees motionless. For some minutes, we conceived that, profiting by the dread exhibited by the workers on guard, she would open it, and destroy the young female; also she prepared to mount the cell; but in doing so she ceased the sound, and quitted the attitude paralysing the bees. The guardians of the cell instantly took courage, and, by means of tormenting and biting the queen, drove her away.

On the fourteenth, the sixth young queen appeared, and the hive threw a swarm, with all the concomitant disorder before described. The agitation was so considerable, that a sufficient number of bees did not remain to guard the royal cells, whence several of the imprisoned queens were enabled to make their escape. Three were in the cluster formed by the swarm, and other three remained in the hive. We removed those that had led the colony, to force the bees to return. They entered the hive, resumed their post around the royal cells, and maltreated the queens when approaching them.

A duel took place in the night of the fifteenth, in which one queen fell. We found her dead next morning before the hive, but three still remained, as one had been hatched during night. Next morning also we saw a duel. Both combatants were extremely agitated, either with the desire of fighting, or by the treatment of the bees when they came near the royal cells. Their agitation quickly communicated to the rest of the colony, which at mid-day departed impetuously along with the two females. This was the fifth swarm that had left the hive between the thirtieth of May and fifteenth of June. On the sixteenth, a sixth swarm cast, of which I shall give you no account, as it showed nothing new.

Unfortunately we lost this last, which was a very strong swarm—the bees flew out of sight, and never could be found. The hive was now very thinly inhabited. Only the few bees that had not participated in the general agitation remained, together with those that returned from the fields after the swarm had departed. The royal cells, therefore, were slenderly guarded ; the queens escaped from them, and engaged in several combats, until the throne pertained to one proving the most successful.

Notwithstanding the victories of this queen, she was treated with great indifference from the sixteenth to the nineteenth, that is, during the three days that she preserved her virginity. At length, having gone forth to seek the males, she returned with all the external signs of fecundation, and was thenceforth received with every mark of respect. Her first eggs were laid forty-six hours after fecundation.

Behold, Sir, a simple and faithful account of my observations on the formation of swarms. That the narrative might be the more connected, I have avoided interrupting it by the detail of several particular experiments which I made at the same time, with the view of elucidating various obscure points of their history. These shall be the subject of future letters; for although I have said so much, I hope still to interest you.—*Pregny*, 6*th September* 1791.

P.S.—In revising this letter, I find I have neglected to anticipate an objection that may embarrass my readers, and which ought to be obviated.

After the first five swarms had thrown, I always returned the bees to the hive, therefore, it is not surprising that it was continually stocked so sufficiently that each colony was numerous. But matters are otherwise in the natural state: the bees composing a swarm do not return to their dwelling; and it will be asked, What resource enables a common hive to swarm three or four times without being too much weakened?

I cannot lessen the difficulty. I have observed that the agitation which precedes swarming is often so considerable, that most of the bees quit the hive; and in that case we cannot well comprehend how, in three or four days afterwards, it can be in a state to send out another colony equally strong.

But remark, in the first place, that the queen leaves a prodigious quantity of workers' brood, which soon transforms to bees; and in this way the population sometimes becomes almost as great after swarming as before it.

Thus the hive is perfectly capable of affording a second colony without being too much impaired.

The third and fourth swarm weaken it more sensibly, but the inhabitants always remain in sufficient number to preserve the course of their labours uninterrupted; and the losses are soon replaced by the great fecundity of the queen, as she lays above an hundred eggs a day.

If, in some cases, the agitation of swarming is so great, that all the bees participate in it, and leave the hive, the desertion lasts but for a moment. The hive throws only during the finest part of the day, and it is then that the bees are ranging through the country. Those that are out, therefore, cannot participate in the agitation ; having returned to the hive, they quietly resume their labours ; and their number is not small, for, when the weather is fine, at least a third are employed in the fields at once.

Even in the most embarrassing case, namely, where the whole bees desert the hive, it does not follow that all those endeavouring to depart become members of the new colony. When this agitation or delirium seizes them, the whole rush forward and accumulate towards the entrance of the hive, and are heated in such a manner that they perspire copiously. Those near the bottom, and supporting the weight of all the rest, seem perfectly drenched—their wings grow moist—they are incapable of flight, and even when able to escape, they advance no farther than the board of the hive, and soon return.

Those that have lately left their cells remain behind the swarm, still feeble, for they could not support themselves in flight. Here then are also many recruits to people what we should have thought a deserted habitation.

LETTER X.

THE SAME SUBJECT CONTINUED.

To preserve greater regularity in continuing the history of swarms, I think it proper to recapitulate, in a few words, the principal points of the preceding letter, and in expatiating on each, to give the result of some new experiments, respecting which I still have been silent.

In the first place, *If, on the return of spring, we examine a hive well peopled, and governed by a fertile queen, we shall see her lay a prodigious number of the eggs of males in the course of May, and the workers will choose that period for constructing several royal cells of the kind described by M. de Reaumur.* Such is the result of several long continued observations, among which there has not been the slightest variation, and I cannot hesitate in announcing it as a fact demonstrated. However, I should here add a necessary explanation. Before a queen commences her *great* laying of the eggs of males, she must be eleven months old—when younger she lays only those of workers. A queen hatched in spring, perhaps will lay fifty or sixty eggs of drones in whole, during the course of the ensuing summer; but previous to beginning her great laying of them, which should be from one to two thousand, she must have completed her eleventh month. In our series of experiments, which more or less disturbed the natural state of things, it often happened that the queen did not attain this age until October, and then immediately began laying male eggs. The workers, as if induced by some emanation from them, also chose the same time for building the royal cells. No swarm resulted thence, it is true, because in autumn all the circumstances requisite are absolutely wanting; but it is not less evident, that there is a secret relation between the production of the eggs of males, and the construction of royal cells.

This laying commonly continues thirty days. The bees on the twentieth or twenty-first lay the foundation of several royal cells. Sometimes they build sixteen or twenty—we have had even twenty-seven. When the cells are three or four lines high, the queen lays those eggs in them from which her own species will come, but not the whole in one day. That the hive may throw several swarms, it is essential that the young females conducting them be not all produced at the same time. It may be affirmed, that the queen anticipates the fact, for she takes care to allow at least the interval of a day between laying every egg deposited in the cells, which is proved by the bees knowing to close them at the moment when the worms are ready to metamorphose to nymphs. Now, as they close all the royal cells at different periods, it is evident the included worms are not all of an equal age.

The queen's belly is extremely turgid before she begins laying the eggs of drones; but sensibly decreases as she advances, and when the laying terminates it is very small. Then she finds herself in a state to undertake a journey which circumstances may prolong; therefore this is a necessary condition; and as every thing is harmonious in the laws of Nature, the origin of the males corresponds with that of the females, which they are to fecundate.

Secondly, *When the larvæ hatched from the eggs laid by the queen in the royal cells are ready to transform to nymphs, this queen leaves the hive, conducting a swarm along with her; and the first swarm that issues from the hive is uniformly conducted by the old queen.** I think I can divine the reason of it.

In order that at no time there may be a plurality of females in a hive, Nature has inspired queens with an innate inveteracy against each other; they never meet without endeavouring to fight, and to accomplish their mutual destruction. Thus, when nearly of equal age, the chance of the contest is equal between them, and fortune will decide to which the empire shall pertain. But if one combatant be older than the rest, she is stronger, and the advantage will be with her. She will destroy her rivals successively as produced. Thence, if the old queen did not leave the hive, before the young ones undergo their last metamorphosis, it could produce no more swarms, and the species would perish. Therefore, to preserve their race, it is necessary that the old queen shall conduct the first swarm. But what is the secret means em-

* Schirach seems to have been aware of the fact now advanced.—*T.*

ployed by Nature to induce her departure? I am ignorant of it.

In this country it is very rare, though not without example, for the swarm led forth by the old queen, in three weeks to produce a new colony, which is also conducted by the same female; and that may happen thus:—Nature has not willed that she shall quit the first hive before her production of male eggs is finished. It is requisite for her to be freed of them, that she may become lighter. Besides, if her first occupation, on entering a new dwelling, was laying more male eggs, still she might perish either from age or accident before depositing those of workers. The bees in that case would have no means of replacing her, and the colony would go to ruin.

All these things have been anticipated with infinite wisdom. The first operation of the bees of a swarm is constructing the cells of workers. They labour at them with great ardour; and as the ovaries of the queen have been disposed with admirable foresight, the first eggs she has to lay in her new abode are those of workers. Commonly her laying continues ten or eleven days; and at this time portions of comb containing large cells are fabricated. We may affirm, that the bees know that their queen will lay the eggs of drones also; she actually does begin to deposit some, though in much smaller number than at first; enough, however, to encourage the bees to construct royal cells. Now, if in these circumstances the weather be favourable, it is not impossible that a second colony may be formed, and that the queen may depart at the head of it within three weeks after conducting the first swarm. But I repeat it, the fact is rare in our climate.

Let me now return to the hives from which the queen has led the first colony.

Thirdly, *After the old queen has conducted the first swarm from the hive, the remaining bees take particular care of the royal cells, and prevent the young queens successively hatched from leaving them, unless at an interval of several days between the departure of each.*

In the preceding letter, I have given you the detail and proof of this fact, and I shall here add some reflections. During the period of swarming, the conduct or instinct of bees seems to receive a particular modification. At all other times, when they have lost their queen, they appropriate workers' worms to replace her; they prolong and enlarge the cells of these worms; they supply them with a greater portion of aliment, and of a more pungent taste; and by this alteration the worms, that would have changed to common bees, are transformed to queens. We have seen twenty-seven royal cells of this kind constructed at once, but when finished, the bees no longer endeavour to preserve the young females from the attacks of their enemies. One perhaps may leave her cell, and assail all the other royal cells successively, which she will tear open to destroy her rivals, without the workers taking any part in their defence. Should several queens be hatched at once, they will pursue each other, and fight until the throne remain with her that is victorious. Far from opposing such duels, the other bees rather seem to excite the combatants.

Matters are quite reversed during the period of swarming. The royal cells then constructed are of a different figure from the former, being fashioned like stalactites, and at first resembling the cup of an acorn. The bees assiduously guard them when the young queens are ready to undergo their last metamorphosis. At length the female hatched from the first egg laid by the old queen leaves her cell; the workers then treat her with indifference. But she, immediately yielding to the instinct which urges her to destroy her rivals, seeks the cells where they are enclosed; yet no sooner does she approach than the bees bite, pull, and harass her, so that she is forced to remove, and, the royal cells being numerous, scarce can she find a place of rest. Incessantly animated with the desire of attacking the other queens, and as incessantly repelled, she becomes agitated, and hastily traverses the different groups of workers, to which she communicates her disorder. At this moment numbers of bees rush towards the aperture of the hive, and, accompanied by the young queen, forsake it to seek another habitation.

After departure of the colony, the remaining workers set another queen at liberty, and treat her with equal indifference as the first. They drive her from the royal cells; she also, perpetually harassed, becomes agitated, departs, and carries a new swarm along with her. In a populous hive this scene is repeated with the same circumstances three or four times during summer. The number of bees being then so much reduced that they are no longer capable of preserving a strict watch over the royal cells, several females are enabled to leave their confinement at once—they seek each other, fight, and the queen, at last victorious, reigns peaceably over the republic.

The longest intervals we have observed between the departure of each natural swarm have been from seven to nine days. This is the time that usually elapses from the period of the first colony being led out by the old queen, until the next swarm is conducted by the first young queen set at liberty. The interval between the second and third is still shorter; and the fourth sometimes departs on the day after the third. In hives left to themselves, fifteen or eighteen days are usually sufficient for the throwing of the four swarms, if the weather continues favourable, as I shall explain.

A swarm is never seen except in a fine day, or, to speak more correctly, at a time of the day when the sun shines, and the air is calm. Sometimes we have observed all the precursors of swarming, disorder, and agitation, but a cloud passed before the sun, and tranquillity was restored—the bees thought no more of their departure. An hour afterwards, the sun having again appeared, the tumult was renewed, it rapidly augmented, and the hive swarmed.

Bees generally seem much alarmed at the prospect of bad weather. While ranging in the fields, the obscuration of the sun by a cloud induces their precipitate return. I am led to think that they are disquieted by the sudden diminution of light. For if the sky is uniformly obscured, and there is no alteration in clearness or by the clouds dispelling, they proceed to the fields for their ordinary collections, nor do the first drops of a soft rain make them return with much precipitation.

I am persuaded that the necessity of a fine day for swarming, is one reason that has induced Nature to admit of bees protracting the captivity of their young queens in the royal cells. I will not deny that they sometimes appear to use this privilege in an arbitrary manner. However, the confinement of the queens is always longer when bad weather lasts several days together. Here the final object cannot be mistaken. Were the young females at liberty to leave their cradles during this intemperate season, there would be a plurality of queens in the hive—consequently combats, and victims would fall. The continuance of bad weather might admit of all the queens undergoing their last metamorphosis at once, or of their having attained their liberty. She proving victorious over the whole would enjoy the throne, and the hive, which should naturally produce several swarms, could give only one. Thus the multiplication of the species would be left to the chance of rain or fine weather, instead of which it is rendered independent of either, by the wise dispositions of Nature. By allowing only a single female to escape at once, the formation of swarms is insured. This explanation appears so simple, that it is superfluous to insist farther on it.

But I should mention another important circumstance resulting from the captivity of queens,—which is, that they are in a condition to fly when the bees have given them liberty, and by this means are capable of profiting of the first moment of sunshine to depart at the head of a colony.

You well know, Sir, that neither drones nor workers are in a state to fly for a day or two after leaving their cells. Then they are of a whitish colour, weak, and their organs infirm. At least twenty-four or thirty hours must elapse before the acquisition of perfect strength, and the development of all their faculties. It would be the same with the females, were not their confinement protracted after the period of transformation ; but we see them appear strong, full-grown, brown, and in a better condition for flying than at any other period. I have elsewhere observed, that constraint is employed to retain the queens in captivity—the bees solder the covering to the sides of the cell by a band of wax. As I have also explained how they are fed, it need not be repeated here.

It is likewise a very remarkable fact, that queens are set at liberty earlier or later according to their age. Immediately when the royal cells were sealed, we marked them all by numbers ; and we chose this period because it indicated the age of the queens exactly. The oldest was first liberated, then the one immediately younger, and so on with the rest. None of the younger queens obtained their freedom before the older ones.

I have a thousand times asked myself how the bees could so accurately distinguish the age of their captives, and undoubtedly I should do better to answer the question by a simple avowal of my ignorance ; at the same time, I must be permitted to offer a conjecture regarding it. You will admit that, as some authors, I have not abused the right of giving myself up to hypothesis. May not the piping or sound emitted by the young queens in their cells be one of the methods employed by Nature to instruct the bees in the age of their females ? It is certain that she whose cell is first sealed is also the first to emit this sound; the queen in the next cell emits it sooner than the rest, and so on those immediately subsequent. I do not deny that, as their captivity may continue eight or ten days, it is possible that the bees in this space of time may forget which has emitted it first; but it is also possible that the queens diversify the sounds, augmenting the loudness as they become older, and that the bees can distinguish these variations. We have been able even ourselves to discern differences in the sound, either with relation to the succession of notes, or their intensity; and probably there are gradations still more imperceptible, escaping our organs, but which may be sensible to those of the workers.

This conjecture receives weight from the queens reared by Schirach's method being perfectly mute; neither do the workers form any guard around their cells, nor do they retain them in captivity a moment beyond the period of transformation; and, having undergone it, they are freely allowed to combat, until one has become victorious over all the rest. Why? —because the object is only to replace the lost queen. Now, provided that, among the worms reared as queens, a single one succeeds, the fate of the others is uninteresting to the bees; whereas, during the period of swarming, it is necessary to preserve a succession of queens for conducting the different colonies; and to ensure the safety of the queens, it is essential to avert the consequences of the mutual antipathy by which they are animated against each other. Behold the evident cause of all the precautions that bees, instructed by Nature, take during the period of swarming,—behold an explanation of the captivity of females,—and why, that the duration of their restraint might be ascertained by the age of the young queens, it was requisite for them to have some method of communicating to the workers when they should be liberated! This method consists in the sound emitted, and the variation they are able to give it.

In spite of all my researches, I never have been able to discover the situation of the organ producing this sound. But I have instituted a new course of experiments on the subject, which are still unfinished.

Another problem still remains for solution. Why are the queens reared, according to Schirach's method, mute, whilst those bred in the time of swarming have the faculty of emitting a certain sound? What is the physical cause of this difference? At first I thought it might be ascribed to the period of life when the worms that are to become queens receive the royal food. While hives swarm, the royal worms receive the food adapted for queens from the moment of leaving the egg;—those, on the contrary, destined for queens, according to Schirach's method, receive it only the second or third day of their existence. It appeared to me that this circumstance might have an influence on the different parts of organization, and particularly on the organ of voice, but experiment has not confirmed my conjecture. I constructed glass cells in perfect imitation of royal cells, that the metamorphosis of the worms into nymphs, and of the nymphs to queens, might be visible. Our experiments are related in a preceding letter. We introduced into one of these artificial cells the nymph of a worm reared according to Schirach's method, twenty-four hours before it should naturally undergo its last metamorphosis; and we replaced the glass cell in the hive, that the nymph might have the necessary degree of heat. On the following day we had the satisfaction of seeing it divest itself of the spoil, and assume its ultimate figure. This queen was prevented from escaping from her prison, but we had contrived an aperture for her thrusting out her trunk, that the bees might feed her. I expected that she would have been completely mute, but it was otherwise; she emitted sounds similar to those already described,—therefore my conjecture was erroneous.

I next conceived that the queen, being restrained in her motions, was induced to emit certain sounds in her desire for liberty. All queens, in this new point of view, should be equally capable of emitting the sound, but, to induce them to it, they must be under restraint. In the natural state, the queens converted from workers are not a single instant confined; and, if they do not emit the sound, it is because nothing impels them to it. On the other hand, those produced at the time of swarming may be induced to do so by the captivity in which they are kept. For my own part, I give little weight to this conjecture; and though I state it here, it is less with a view to claim merit than to put others on a plan of discovering something more probable.

I do not ascribe to myself the credit of having discovered the piping of the queen bee. Old authors speak of it. M. de Reaumur cites a Latin work published 1671, *Monarchia Femina*, by Charles Butler.*

* This work was published so long ago as 1634. It is both written and printed in the most extravagant style.—*T.*

He gives a very brief abstract of this naturalist's observations, who, we can easily see, has exaggerated or rather disguised the truth, by mixing it with the most absurd fancies; but it is not the less evident that Butler has heard this peculiar piping of queens, and that he did not confound it with the confused humming sometimes heard in hives.

Fourthly, *Young queens conducting swarms from their native hive are still in a virgin state.* They generally depart in quest of the males the day after being settled in their new abode, which is usually the fifth of their existence as queens, for two or three are passed in captivity—one in their native hive, and a fifth in their new dwelling. Those queens that come from the worm of a worker also pass five days in the hive before going in quest of males. So long as in a state of virginity, both are treated with indifference by the bees, but after returning with the external marks of fecundation, they are received by their subjects with the most distinguished respect. However, forty-six hours elapse after fecundation before they begin to lay. The old queen, which leads the first swarm in spring, requires no farther commerce with the males for the continuance of her fecundity. Union only once is sufficient to impregnate all the eggs that she will lay for at least two years.—*Pregny, 8th September* 1791.

LETTER XI.

THE SAME SUBJECT CONTINUED.

My chief observations on swarms, those most frequently repeated, and of which the uniformity of result leads me to apprehend no error, are collected in the two preceding letters. I have deduced what seem the most direct consequences, and in all the theoretical part I have sedulously avoided going beyond facts. What remains to be mentioned is more hypothetical, but it engrosses several curious experiments.

It has been demonstrated that the principal motive urging the departure of the young females when hives swarm, is their insuperable antipathy to each other. I have repeatedly observed that they cannot gratify their aversion, because the workers with the utmost care prevent them from attacking the royal cells. This perpetual opposition at length creates a visible inquietude, and excites a degree of agitation that induces them to depart. All the young queens are successively treated alike in hives that are to swarm, but the conduct of the bees towards the old queen destined to conduct the first swarm is very different. Always accustomed to respect fertile queens, they do not forget what they owe to her; they allow her the most uncontrolled liberty. She is permitted to approach the royal cells, and, if she even attempts to destroy them, no impediment is offered by the bees. Thus her inclinations are not obstructed, and we cannot ascribe her flight, as that of the young queens, to the opposition she suffers. Therefore, I candidly confess myself ignorant of the motives of her departure.

Yet, on more mature reflection, it does not appear to me that this fact affords so strong an objection against the general rule as I had at first conceived. It is certain, at least, that the old queens as well as the young ones entertain the greatest aversion towards the individuals of their own sex. This has been proved by the numerous royal cells destroyed. You will remember, Sir, that in my first observations on the departure of old queens, seven royal cells opened at one side were destroyed by the queen. If rain continue several days, destruction awaits the whole, in which case there is no swarm, as too often happens in our climate where spring is generally wet. Queens never attack cells containing an egg or a very young worm, but only when the worm is ready for transforming to a nymph, or when it has undergone its last metamorphosis.

The presence of royal cells with nymphs or worms near their change also inspires old queens with the utmost horror or aversion, but here it would be necessary to explain why the queen does not always destroy them though it be in her power. On this point I am limited to conjectures. Perhaps the great number of royal cells in a hive at once, and the labour of opening the whole, creates insuperable alarm in the old queen. She commences indeed with attacking her rivals, but, incapable of obtaining immediate success, her disquiet during the work is roused to a terrible agitation. If the weather continues favourable while she remains in this condition she is naturally disposed to depart.

It may be easily understood that the workers, accustomed to respect their queen, whose presence is a real necessity to them, crowd after her—the formation of the first swarm would create no difficulty in this view. But undoubtedly you will ask, Sir, what motive can induce the workers to follow their young queens from the hive, while they maltreat them so much, and even, in their most amiable moments, testify perfect indifference towards them? Probably it is to escape the heat to which the hive is then exposed. The extreme agitation of the females leads them to traverse the combs in all directions. The groups of bees through which they pass are injured and deranged, a kind of delirium is imparted to them, and this tumultuous disorder raises the temperature to an insupportable degree. We have frequently proved it by the thermometer. In a populous hive it commonly stands between 90 deg. and 97 deg., in a fine day of spring, but during the tumult which precedes swarming it rises above 104 deg., and this is heat intolerable to bees. When exposed to it, they rush impetuously towards the outlets of the hive and depart. In general they cannot endure the sudden augmentation of heat and then quit their dwelling, neither do those returning from the fields enter when the temperature is extraordinary.

By means of direct experiments I have ascertained that the impetuous courses of the queen over the combs actually throws the workers into a state of agitation. It was particularly important to learn whether she would communicate her disorder independent of the time of swarming. But I wished to avoid a complication of causes. I took two females, still virgins, of above five days old, susceptible of fecundation, and, having put them into separate glass hives sufficiently populous, where the air had free circulation, effectually precluded any of the bees from escaping. Then I prepared to observe the hives every moment that the fineness of the weather might invite both males and females to go abroad for the purpose of fecundation. Next morning being gloomy, no male left the hive, and the bees were tranquil; but towards eleven of the following day, the sun shining bright, both queens began to run about seeking an exit from every part of their dwelling, and from their inability to find one, traversed the combs with the most evident symptoms of disquiet and agitation. The bees soon participated of the same disorder; they crowded towards the lower part of the hive, where the openings were situated; unable to escape, they ascended with equal rapidity, and hurried heedlessly over the cells until four in the afternoon. It is nearly about this hour that the sun declining in the horizon recalls the males; queens requiring fecundation never remain later abroad. The two females became calmer, and tranquillity in a short time was restored. This was repeated several subsequent days with perfect similarity, and I am now convinced that there is nothing singular in the agitation of bees while swarming, but that they are always in a tumultuous state when the queen herself is in agitation.

I have but one fact more to mention. It has been already observed, that on losing the female, bees give the larvæ of simple workers the royal treatment, and, according to Schirach, in five or six days they repair the loss of their queen. In this case there are no swarms. All the females leave their cells almost at the same moment, and, after a sanguinary combat, the throne remains with the most fortunate.

I can comprehend very well that the object of Nature is to replace the lost queen; but as bees are at liberty to choose either the eggs or worms of workers during the first three days of existence to supply her place, why do they give the royal treatment to worms all of nearly an equal age, and which must undergo their last metamorphosis almost at the same period? Since they are enabled to retain the young females in their cells, why do they allow all the queens, reared according to Schirach's method, to escape at once? By prolonging their captivity more or less, they would fulfil two most important objects at the same time, in repairing the loss of their females, and in preserving a succession of queens to conduct several swarms.

At first it was my opinion that this difference of conduct proceeded from the difference of circumstances in which they find themselves situated. They are induced to make all their dispositions relative to swarming only when in great numbers, and when they have a queen occupied with her principal laying of male eggs; whereas, having lost their female, the eggs of drones are no longer in the combs to influence their instinct. They are in a certain degree restless and discouraged.

On this account, after removing the queen from a hive, I thought of rendering all the other circumstances as similar as possible to the condition of bees preparing to swarm. By introducing a great many workers, I increased the population to excess, and supplied them with combs of male brood in every stage. Their first occupation was constructing royal cells after Schirach's method, and rearing common worms with royal food. They also began some stalactite cells, as if the presence of the male brood had inspired them to it; but these they discontinued, as there was no queen to deposit her eggs. Finally, I gave them several close royal cells, taken indifferently from hives preparing to swarm. However, all my precautions were fruitless; the bees were occupied only with replacing their lost queen; they neglected the royal cells entrusted to their care; the included queens came out at the ordinary time, without being detained prisoners a moment; they engaged in several combats, and there were no swarms.

Recurring to subtleties, we may perhaps suggest the cause of this apparent contradiction. But the more we admire the wise dispositions of the Author of Nature in the laws he has prescribed to the industry of animals, the greater reserve is necessary in admitting any theory adverse to this beautiful system, and the more must we distrust that facility of imagination from which we think by embellishment to attain the elucidation of facts.

Naturalists in general, who have made animals the subject of long observation, and those in particular who have chosen insects for their favourite study, have too readily ascribed to them our sentiments and passions, and even our designs. Yielding to admiration, and disgusted perhaps by the contempt with which insects are treated, they have conceived themselves obliged to justify the consumption of time bestowed on this pursuit, and they have depicted different traits of the industry of these minute animals, with the colours inspired by a warm imagination; nor is the celebrated Reaumur even to be acquitted of such a charge. He frequently ascribes combined intentions to bees—love, anticipation, and other faculties of too elevated a kind. I think I can discern that, although he formed a very just conception of their operations, he would be well pleased that his reader should admit that they are sensible of their own interest. He is a painter who by a happy prejudice flatters the original, whose features he depicts. On the other hand, Buffon unjustly considers bees as mere automatons. It was reserved for you, Sir, to establish the theory of animal industry on the most philosophical principles, and to demonstrate that those actions that have a moral appearance depend on an association of ideas *simply sensible.* It is not my object here to penetrate such depths, or to insist on the details.

But, on the whole, the facts relative to the formation of swarms, perhaps present more subjects for admiration than any other part of the history of bees; whence I think it proper to state, in a few words, the simplicity of the methods by which the wisdom of Nature guides their instinct. It cannot allow them the slightest portion of understanding—it leaves them no precautions to be taken, no combination to be followed, no foresight to exercise, no knowledge to acquire. But their sensorium being adapted to the different operations with which they are charged, it is the impulse of pleasure which leads them on. She has therefore pre-ordained all that is relative to the succession of their different labours, and, to each operation she has united an agreeable sensation. Thus, when bees construct cells, watch over their larvæ, and collect provisions, we must not ascribe it to method, affection, or foresight. The only inducement must be sought for in some pleasing sensation attached to each of these functions. I address a philosopher; and as these are his opinions applied to new facts, I believe my language will be easily understood. But I request my readers to peruse and to reflect on that part of your works which treats of the industry of animals. Let me add but another sentence. The impulse of pleasure is not the sole agent—there is another principle, the extraordinary influence of which, at least with regard to bees, has been hitherto unknown, that is, the sentiment of aversion continually felt by all females against each other—a sentiment whose existence is so fully demonstrated by my experiments, and which explains many important facts in the theory of swarms.—*Pregny, 10th September* 1791.

LETTER XII.

ADDITIONAL OBSERVATIONS ON QUEENS LAYING ONLY THE EGGS OF DRONES, AND ON THOSE DEPRIVED OF THE ANTENNÆ.

In relating my first observations on queens laying male eggs alone, I have proved that they deposit them in cells of all dimensions indifferently, and even in royal cells. It is also proved that the same treatment is given to male worms hatched from eggs laid in the royal cells, as if they were actually to be transformed to queens; and I have added, that in this instance the instinct of the workers appeared defective. It is indeed most singular, that bees, knowing the worms of males so well when the eggs are laid in small cells, and never failing to give them a convex covering when about to transform to nymphs, should no longer recognise the same species of worms when the eggs are laid in royal cells, and treat them exactly as if they should change to queens. This inconsistence depends on something incomprehensible by me.*

* I have ascertained, from new observations, that bees recognise the larvæ of males, as well when the eggs producing them have been laid in royal cells by queens of retarded fecundation, as when deposited in common cells.

The former, it will be recollected, resemble a pear with the

In revising what is said on the subject, I observed an interesting experiment still wanting to complete the history of queens laying only the eggs of drones. I had to investigate whether they could themselves

small end downwards, and are an inch and a quarter, or little more in length. When only sketched, and very like the cup of an acorn, queens lay in them.

Bees, indeed, give the same shape and dimensions to those serving as receptacles for the males, but when the transformation of the larvæ approaches, it is easy to discover that they have not taken them for royal worms. Instead of closing their cells in a pointed form, as they do invariably if containing the larvæ of queens, they widen the extremity, and adding a cylindrical tube, shut them with a convex covering differing in nothing from that which they are accustomed to put on the cells of males. But as this tube is of the same capacity as the hexagonal cells of the smallest diameter, the worm, which is forced by the bees to descend into it, and which must there undergo its final metamorphosis, becomes a drone of the smallest size. The total length of these extraordinary cells is from 20 to 22 lines (1 2-3d to 1 5-6th inch): yet bees do not always add a cylinder to a pyramidal cell; they are content with giving the lower part a little enlargement, and, here the growing larva may become a large drone.

I am ignorant what is the cause of a difference sometimes observed in the form of these cells; but it appears very certain to me, that the bees never are deceived in them; thus affording a great proof of the instinct with which they are endowed.

Bees being intrusted by Nature with bringing up their young, and with the care of providing them with the aliment proper to their age, or even to their sex, should be also endowed with the faculty of recognising them. There is so little resemblance between the adult male and workers, that some difference should subsist between their respective larvæ. Doubtless the workers distinguish it, though escaping our notice.

distinguish that the eggs which they deposit in the royal cells would not produce queens. I have already remarked that they do not endeavour to destroy these cells when close, and I thence concluded, that in general the presence of royal cells in their hive does not inspire them with the same aversion to females whose fecundation has been retarded; but to ascertain the fact more correctly, it was essential to examine how the presence of a cell containing a royal nymph would affect a queen that had never laid any other than the eggs of drones.

The experiment was easy, and I put it in practice on the fourth of September, in a hive some time deprived of its queen, where the bees had not failed to construct several royal cells for replacing her. I chose this opportunity to supply them with a female, whose fecundation had been retarded to the twenty-eighth day, and which laid none but the eggs of males. At the same time, I removed all the royal cells, except one that had been sealed five days. A single cell remaining was enough to show the impression it would make on the stranger queen introduced. If she endeavoured to destroy it, in my opinion, this would prove that she anticipated the origin of a dangerous rival. You must admit the use I now make of the word anticipate; it is to avoid circumlocution; I feel its impropriety. If, on the contrary, she did not attack the cell, I would thence infer that the postponed fecundation, which deprived her of the power of laying workers' eggs, had also impaired her instinct. The latter was the fact. The queen passed several times over the royal cell, both the first and the subsequent day, without seeming to distinguish it from the rest. She quietly laid in the surrounding cells; notwithstanding the cares incessantly bestowed by the bees upon it, she never one moment appeared to suspect the danger with which the included royal nymph threatened her; besides, the workers treated their new queen as well as they would have treated any other female. They were lavish of honey and respect, and formed those regular circles around her that seem an expression of homage.

Thus, independent of the derangement occasioned by retarded impregnation in the sexual organs of queens, it certainly impairs their instinct. Aversion or jealousy against their own sex in the nymphine state ceases, nor do they endeavour to destroy them in their cradles.

My readers will be surprised that queens, whose fecundation has been retarded, and whose fecundity is so useless to bees, nevertheless should be so well treated, and become as dear to them as females laying both kinds of eggs. But I remember to have observed a fact more astonishing still. I have seen workers bestow every attention on a queen though sterile, and after her death treat her dead body as they had treated herself when alive, and long prefer it, though inanimate, to the most fertile queens I had offered them. This sentiment, which assumes the appearance of lively affection, is probably the effect of some agreeable sensation communicated to bees by their queen, independent of fertility. Those laying only the eggs of males undoubtedly excite the same sensation in the workers.

I now recollect that the celebrated Swammerdam somewhere observes, that when a queen is blind, sterile, or mutilated, she ceases to lay, and the workers of her hive no longer continue their labours or make any collections, as if aware that it is useless to do so. But in advancing this as a fact, he cites no experiments that led him to the discovery. Those made by myself have afforded some very singular results.

I frequently amputated the four wings of queens; yet not only did they continue laying, but the same consideration of them was testified by the workers as before. Therefore, Swammerdam has no foundation for asserting that mutilated queens cease to lay. Indeed, from his ignorance of fecundation taking place without the hive, it is possible that he cut the wings off virgin queens, and they, becoming incapable of flight, remained barren from inability to seek the males in the air. Thus amputation of the wings does not produce sterility.

I have frequently cut off one of the antennæ to recognise a queen the more easily, and it was not prejudicial to her either in fecundity or instinct, nor did it affect the attention paid to her by the bees. It is true, that as another still remained, the mutilation was imperfect, and nothing was decided by the experiment. But amputation of both antennæ produced most singular effects. On the fifth of September, I cut them off a queen that laid the eggs of males only, and put her into the hive immediately after the operation. From this moment there was a great alteration in her conduct. She traversed the combs with extraordinary vivacity, scarcely had the workers time to separate and recede before her, and she dropped her eggs, without the precaution of depositing them in any cell. The hive not being very populous, part wanted combs. Hither she seemed particularly earnest to repair, and remained long motionless. She appeared to avoid the bees; however, several workers followed her into this solitude, and treated her with the most evident respect. She seldom required honey from them, but, when that occurred, she directed her trunk with an uncertain kind of feeling, sometimes on the head and sometimes on the limbs of the workers, and if it did reach their mouths, it was by chance. At other times she returned upon the combs, then quitted them to traverse the glass sides of the hive, and always dropped eggs during her various motions. Sometimes she appeared tormented with the desire of leaving her habitation, and rushing towards the opening, entered the glass tube adapted there; but the external orifice being too small, after fruitless exertion, she returned. Notwithstanding these symptoms of delirium, the bees did not cease to render her the same attention as they ever pay to their queens, but this one received it with indifference. All that I describe appeared to me the consequence of amputating the antennæ. However, her organization having already suffered from retarded fecundation, and as I had observed her instinct in some degree impaired, both causes might possibly concur in operating the same effect. To distinguish what peculiarly belonged to privation of the antennæ, repetition of the experiment was necessary on a queen otherwise well organized, and capable of laying both kinds of eggs.

This was practised on the sixth of September. I amputated both the antennæ of a female which had been several months the subject of observation, and, being of great fecundity, had already laid a considerable number of workers' eggs, and those of males. I put her into the same hive where the queen of the preceding experiment still remained, and she exhibited precisely the like symptoms of delirium and agitation, which I think it needless to repeat. I shall only add that, to judge better of the effect produced by privation of the antennæ on the industry and instinct of bees, I attentively considered the manner in which these two mutilated queens treated each other. You cannot have forgot, Sir, the animosity with which queens, possessing all their organs, combat; therefore, it became extremely interesting to learn whether a similar reciprocal aversion would subsist after losing their antennæ. We studied these females a long time; they met several times in their courses, and without exhibiting the smallest resentment. This last instance is, in my opinion, the most decisive evidence of a change operated in their instinct.

Another very remarkable circumstance, which the same experiment gave me occasion to observe, consisted in the good reception given by the bees to the stranger queen, while they still preserved the first. Having so often seen the symptoms of discontent occasioned by a plurality of queens, after having witnessed the clusters formed around such supernumeraries to confine them, I could not expect that they would pay the same homage to a second mutilated one which they still testified towards the first. Was it from losing the antennæ that these queens no longer retained any characteristic distinguishing the one from the other?

I was the more inclined to admit this conjecture, from the bad reception of a third fertile queen preserving her antennæ, introduced into the same hive. The bees seized, bit her, and restrained her so closely that she could hardly breathe or move. Therefore, if they treat two females deprived of the antennæ equally well in the same hive, it is probably because they experience the same sensation from both, and want the means of continuing to distinguish them from each other.

From all this, I conclude, that the antennæ are not a frivolous ornament to insects, but, according to every appearance, are the organs of touch or smell. Yet I cannot affirm which of these senses reside in them. It is not impossible that they are organized in such a manner as to fulfil both functions at once.

As in the course of the last experiment the two mutilated females constantly endeavoured to escape from the hive, I wished to see what they would do if set at liberty, and whether the bees would accompany them in their flight. Therefore, leaving the fertile mutilated one, I removed the first and third from the hive, and then enlarged the entrance.

That day the queen left her habitation. At first she tried to fly, but, her belly being full of eggs, she fell down, and never attempted it again. No workers accompanied her. Why, after rendering the queen so much attention while she lived among them, did they abandon her now on her departure? You know, Sir, that queens governing a weak swarm are sometimes discouraged, and fly away, carrying all their little colony along with them. In like manner sterile queens, and those whose dwelling is ravaged by weevils, depart, and are followed by all their bees. Why, therefore, in this experiment, did the workers allow their mutilated queen to depart alone? All that I can hazard on the question is a conjecture. It appears that bees are induced to quit the hives from the increased heat, which occasions the agitation of their queen, and the tumultuous disorder which she communicates to them. A mutilated queen, notwithstanding her delirium, does not agitate the workers, because she seeks the glass panes and the uninhabited parts of the hive—she hurries over clusters of bees, but the shock resembles that of any other body, and produces only a local and instantaneous motion. The sensation arising from it is not communicated from one place to another, like that occasioned by a queen which in the natural state wishes to abandon her hive and lead out a swarm; there is no augmented temperature, consequently nothing that renders the hive insupportable to the bees.

This conjecture, which affords a tolerable explanation why bees persist in remaining in the hive though the mutilated queen has left it, does not account for the motive inducing the queen herself to depart. Her instinct is altered, but that is all of which I am sensible. I can discern nothing more. It is very fortunate for the hive that she quits it, for the bees incessantly attend her, nor do they ever think of procuring another queen while she remains; and if she were long of leaving them, it would be impossible to replace her, for the workers' worms would exceed the term at which they are convertible into royal worms, and the hive would perish. Observe, that the eggs dropped by the mutilated queen can never serve to replace her, for, not being deposited in cells, they wither and produce nothing.

I have yet to say a few words on the females laying male eggs only. Schirach supposes that one branch of their double ovary suffers some alteration. He seems to think that the eggs of males are contained in one of the branches, while the other has none but common eggs; and, as he ascribes the incapacity of certain queens to lay the latter to some disease, his hypothesis certainly is very plausible. In fact, if the eggs of males and workers are indiscriminately mixed in both branches of the ovary, it appears, at first sight, that whatever may be the cause acting on this organ, it should affect both species of eggs equally. If, on the contrary, one branch is occupied by the eggs of drones only, and the other contains none but common eggs, we may conceive how disease affects the one, while the other remains untouched. Notwithstanding the probability of this conjecture, it is confuted by observation. We lately dissected queens which laid none but male eggs, and found both branches of the ovary equally well expanded, and equally sound, if I may use the expression. The only difference of which we were sensible consisted in the eggs not lying so close together, apparently, in these two branches, as in the ovaries of queens by which both kinds are laid. —*Pregny*, 12*th September* 1791.

LETTER XIII.

ECONOMICAL CONSIDERATIONS ON BEES.

In this letter I shall treat of the advantages that may be derived from the new invented hives, called *book* or *leaf* hives, in promoting the *economical knowledge* of bees.

It is needless to relate the different methods hitherto employed in compelling these insects to yield up a portion of their honey and wax—all correspond in being cruel and ill understood.

It is evident that when bees are cultivated for the purpose of sharing the produce of their labours, we must endeavour to multiply them as much as the nature of the country admits, consequently we ought to regard their lives at the time we plunder them. Therefore, it is an absurd proceeding to sacrifice whole hives to get at the riches they contain. The inhabitants of this country, who follow no other method, annually lose immense numbers of hives; and spring being generally unfavourable to swarms, the loss is irreparable. I know well that at first they will not adopt a different plan—they are too much attached to prejudices and old customs, but naturalists and intelligent cultivators of bees will be sensible of the utility of the method I propose, and, if they apply it to use, I hope their example will extend and perfect the culture of bees.

It is not more difficult to lodge a natural swarm in a leaf hive than in any other of a different shape; but there is one precaution essential to success, which I should not omit adverting to. Though bees are indifferent as to the position of their combs, and their greater or lesser size, they are obliged to construct them perpendicular to the horizon, and parallel to each other. Therefore, if left entirely to themselves, when establishing a colony in one of these new hives, they will frequently construct several small combs, parallel indeed, but perpendicular to the plane of the frames or leaves, or perhaps they will build them on the place where two are joined together, and by this disposition prevent the advantages which I think to derive from the figure of my hives, since they could not be opened without breaking the combs.* Thus they must previously have a guide; the cultivator himself can lay the foundation of their edifices, and that by a simple method. A portion of comb must be fixed securely in the upper part of some of the frames composing the hive—the bees will extend it, and, in prosecution of their work, will accurately follow the plan already given them.† Therefore, on opening the hive, no obstacle is to be removed, nor

* The author's general meaning is, the bees will build across the hive, or parallel to the narrow sides of the frames.—*T.*

† If the upper part of a frame form a salient angle, it is said that bees establish their original work with greater regularity.—*T.*

are stings to be dreaded, for one of the most singular and valuable properties attending this construction, is its rendering the bees tractable. I appeal to you, Sir, for the truth of what I say. In your presence I have opened all the divisions of the most populous hives, when the tranquillity of the bees has given you great surprise. I can desire no other evidence of my assertion. It is in the facility of opening these hives at pleasure that all the advantages are concentrated, which I expect in perfecting the economical knowledge of bees.

When I observe that bees may be rendered tractable, I conceive it need not be added that I do not arrogate to myself the absurd pretence of *taming* them, for this excites a vague idea of deception, and I would willingly avoid the hazard of exposing myself to any such reproach. I ascribe their tranquillity, on opening the hives, to the manner in which they are affected by the sudden introduction of light—then they appear rather to testify fear than anger. Many retire, and entering the cells seem to conceal themselves. My conjecture is confirmed by their being less tractable during night or after sunset than through the day. Thus we must open the hives, while the sun is above the horizon, cautiously and without any sudden shock —separating the divisions slowly, and taking care to wound none of the bees. If they cluster too much on the combs, it is necessary to brush them off with a feather, and breathing on them is to be anxiously avoided. The air which we expire seems to excite their fury—it certainly possesses some irritating quality, for, if bellows be used, they are rather inclined to escape than to sting.

Respecting the advantages of leaf hives, I shall remark, that they are extremely convenient for forming *artificial* swarms. In the history of *natural* swarms, I have shown how many favourable circumstances are requisite for their success. From experience I know that they very often fail in our climate ; and even when a hive is disposed to swarm, it frequently happens that the colony is lost, either because the moment of its departure has not been foreseen, because it rises out of sight, or settles on inaccessible places. Instructing the cultivators of bees how to make artificial swarms is a real benefit, and the form of my hives renders this an easy process. But it demands farther illustration.

Since bees, according to Schirach's discovery, can procure another queen after having lost their own, provided there be workers' brood in the combs not above three days old, it results that we can produce queens at pleasure, by removing the reigning one. Therefore, if a hive sufficiently populous be divided in two, one half will retain the old queen, and the other will not be long of obtaining a new one. But to ensure success, we must choose a propitious moment, which is never certain but in leaf hives. In these we can see whether the population is sufficient to admit of division, if the brood is of the proper age, and if males exist or are ready to be produced for impregnating the young queens.

Supposing the concurrence of all these conditions, the following is the method to be pursued. The leaf hive is to be separated through the middle without any shock, and two empty divisions insinuated between the halves, which, when exactly applied to each other, are close on the outside. The queen must be sought in one of the halves, and marked to avoid mistake. If she by chance remains in the division with most brood, she is to be transferred to the other with less, that the bees may have every possible opportunity of obtaining another female. Next, it is necessary to connect the halves together, by a cord tied around them; and care must be taken that they are set on the same board that the hive previously occupied. The old entrance, now become useless, will be shut up; but as each half requires a new one, it ought to be made at the bottom of the first and twelfth division, on purpose that they may be as far asunder as possible. Both entrances should not be opened on the same day. The bees in the half deprived of the queen are to be confined twenty-four hours, and no aperture made before that time elapses except for the admission of air. Without this precaution, they would soon search after their queen, and infallibly finding her in the other division, they would then retire in great numbers from their own, until too few remained to perform the necessary labours. But this will not ensue if they be confined twenty-four hours, provided that interval is sufficient to make them forget the queen. When all circumstances are favourable, the bees, in the division wanting her, will begin the same day to labour in procuring another, and ten or fifteen days after the operation their loss will be repaired. The young female they have reared soon issues forth to seek impregnation, and in two days commences the laying of workers' eggs. Nothing more is wanting to the bees of this half hive—and the success of the artificial swarm is ensured.

It is to Schirach that we are indebted for such an ingenious method of forming swarms. He supposes that, by producing young queens in the beginning of spring, early swarms might be procured, which certainly would be advantageous in most situations. But unfortunately this is impossible. Schirach believed that queens were fecundated of themselves, consequently he thought that, after being artificially produced, they would lay and give birth to a numerous posterity. Now, this is an error: the females, to become fertile, require the concourse of the males, and if not impregnated within a few days of their origin, their laying, as I have observed, is completely deranged. Therefore, if a swarm were artificially formed before the usual time of the males originating, the bees would be discouraged by the sterility of the young female; or should they remain faithful to her, awaiting the period of fecundation, as she could not receive the approaches of the male for three or four weeks, she would lay eggs producing males only, and the hive in that case would perish. Thus the natural order must not be deranged, but the division of hives delayed until males are about to originate or actually exist.

Besides, if Schirach did succeed in obtaining artificial swarms, notwithstanding the great inconvenience of his hives, it was owing to his singular address and unremitting assiduity. He had some pupils in the art, by whom it was communicated to others; and there are people now in Saxony who traverse the country practising this operation. Those versant in the matter can alone venture to undertake it with common hives, whereas every cultivator can do it himself with the leaf hives.

In this construction, another very essential advantage will be found. Bees can be forced to work in wax; and here I am led to what I believe is a new observation:—While naturalists have directed our admiration to the parallel position of the combs, they have overlooked another trait in the industry of bees, namely, the equal distance uniformly preserved between them. On measuring the interval by which they are separated, in general it will be found a third of an inch. Were they too distant, it is very evident that the bees would be greatly dispersed, and unable to communicate their heat reciprocally, whence the brood could not receive sufficient warmth. Were the combs too close, on the contrary, the bees could not freely traverse the intervals, and the work of the hive would suffer. Therefore, a certain distance always uniform is requisite, which is equally adapted to the service of the hive, and the care necessary for the worms. Nature, which has taught bees so much, has instructed them in the regular preservation of this distance. On the approach of winter, they sometimes elongate the cells which are to contain honey, and thus contract the intervals between the combs. But this expedient is in preparation for a season, when it is important to have copious magazines, and when, their activity being relaxed, it is unnecessary for their communications to be so spacious and free. On the return of spring the bees hasten to contract the elongated cells, that they may become fit for receiving the eggs which the queen will lay, and thus re-establish the suitable distance which Nature has ordained.

This being premised, bees may be forced to work in wax, or, which is the same thing, to construct new combs. To accomplish the object, it is only necessary to separate those already built so far asunder that they may construct others in the interval. Suppose that an artificial swarm is lodged in a leaf hive, composed of six divisions, each containing a comb, if the young queen is as fertile as she ought to be, the bees will be very active in their labours, and disposed to make great collections of wax. To induce them towards it, an empty frame or division must be inserted between two others, each containing a comb. As all the frames are of equal dimensions, and of the necessary width for receiving a comb, the bees having sufficient space for constructing a new one in the empty division introduced into the hive, will not fail to build it, because they are under the necessity of never leaving more than a third of an inch between them. Without any guide, this new comb will be parallel to the old ones, to preserve that law which establishes an equal distance throughout the whole surface.

If the hive is strong and the weather good, three empty divisions may be left at first between the old combs; one between the first and second, another between the third and fourth, and the last between the fifth and sixth. The bees will fill them in seven or eight days, and the hive will then contain nine combs. Should the temperature of the weather continue favourable, three new leaves or divisions may be introduced, consequently, in fifteen days, or three weeks, the bees will have been forced to construct six new combs. The experiment may be extended farther in warm climates, and in those where flowers perpetually blow. But in our country, I have reason to think that the labour should not be forced more during the first year. Plate I. fig. 1, 2.

From these details, you are sensible, Sir, how preferable *leaf hives* are to those of any other construction, and even to the ingenious stages described by *M. Palteau*; for the bees cannot be compelled by means of the latter to labour more in wax than they would if left to themselves; whereas they are obliged to do it by inserting empty divisions in the former. Neither can the combs constructed on those stages be removed without destroying considerable portions of brood, deranging the bees, and creating real disorder in the hive.

Mine have also this advantage, that what passes within may be daily observed, whence we can judge of the most favourable moments for depriving the bees of part of their stores. With all the combs before us, we can distinguish those containing brood only, and what it is proper to preserve. The scarcity or abundance of provisions is exposed, whence the portion that is suitable may be taken away.

I should protract this letter too much were I to give an account of all my observations on the time proper for inspecting hives, on the rules to be followed in the different seasons, and the proportion to be observed in dividing the riches of bees with themselves. The subject would require a separate work; and, perhaps, I may one day engage in it; but until that arrives, I shall always feel gratification in communicating to cultivators wishing to follow my method, those directions of which long practice has demonstrated the utility.

Here I shall only observe that we hazard absolute ruin of the hives, by robbing them of too great a proportion of honey and wax. In my opinion, the art of cultivating these insects consists in moderately exercising the privilege of sharing their labours; but in compensation for it, every method must be employed which promotes the multiplication of bees. Thus, for example, if we desire to procure a certain quantity of honey and wax annually, it will be better to seek it in a number of hives, managed with discretion, than to plunder a few of a great proportion of their treasures.

It is indubitable that the multiplication of these industrious creatures is greatly injured by privation of several combs, in a season unfavourable to the collection of wax, because the time occupied in replacing them is taken from that which should be consecrated to the care of the eggs and worms, and by this means the brood suffers. Besides, they must always have a sufficient provision of honey left for winter; for although less is consumed during that season, they do consume a certain quantity; because they are not torpid, as some authors have conceived.*

* So far from being torpid in winter, when the thermometer in the open air is several degrees below freezing, it stands at 86 deg. and 88 deg. in hives sufficiently populous. The bees then cluster together, and move to preserve their heat.

Swammerdam was of the same opinion, and observes, that the heat of a hive is such, even in the middle of winter, that the honey never crystalizes, or assumes a granulated consistence, unless the numbers of the bees be too much reduced. Farther, that when their queens are very fertile they feed their young with honey also in the middle of winter, watching over and heating them, and also heating each other. "I know not," he continues, "whether there are other insects having similar

Therefore, if they have not enough, they must be supplied with it, which requires a very exact proportion. I admit, that in determining to what extent hives may be multiplied in a particular country, it is necessary first to know how many that country can support, which is a problem yet unsolved. It also depends on another problem, the solution of which is as little known, namely, the greatest distance that bees fly to in collecting their provender. Different authors maintain, that they can fly several leagues from the hive. But according to the few observations I have been able to make, this distance seems greatly exaggerated. It appears to me that the radius of the circle

properties in common with bees: for hornets, wasps, and humble bees, as well as flies, &c., remain torpid and motionless during the whole winter."

In the month of January, M. de Reaumur found brood of all ages in certain hives. The same thing happened to myself, when the thermometer stood in my hives at about 93 deg.

Now that I am on the subject of thermometrical observations, I may cursorily remark, that M. Dubost of Bourg en Bresse, in a memoir otherwise valuable, is of opinion that the larvæ cannot be hatched below 104 deg. I have repeatedly made the experiment with the most accurate thermometers, and obtained a very different result. When the thermometer rises to 104 deg., the heat is so much greater than the eggs require that it is intolerable to the bees. M. Dubost has been deceived, I imagine, by introducing his thermometer too suddenly into a cluster of bees, and the mercury has risen higher, from the agitation excited among them, than it should naturally do. Had he kept it there, he would have soon seen it fall to between 95 and 97 deg., which is the usual temperature of hives in summer. In August this year, when the thermometer in the open air stood at 94 deg., it did not rise above 99 deg. in the most populous hives. The bees had little motion, and a great many rested on the board of the hive.

they traverse does not exceed half a league; and as they return to the hive with the greatest precipitation whenever a cloud passes before the sun, it is probable that they do not fly far. Nature, which has inspired them with such terror for a storm, and even for rain, undoubtedly restrains them from going so far as to be too much exposed to the injuries of the weather. I have endeavoured to ascertain the fact more positively, by transporting to various distances bees with the thorax painted, that they might be again recognised. But none ever returned that I had carried for twenty-five or thirty minutes from their dwelling, while those at a shorter distance have found their way back. I do not state this experiment as decisive. Though bees generally do not fly above half a league, it is very possible that they go much farther when flowers are scarce in their own vicinity. An experiment to be conclusive must be made in vast arid or sandy plains, separated by a known distance from a fertile region.

Thus the question yet remains undecided. But without pronouncing on the number of hives that any district can maintain, I shall remark, that certain vegetable productions are much more favourable to bees than others. More hives, for example, may be kept in a country abounding in meadows, and where black grain is cultivated, than in a district of vineyards or corn.

Here I terminate my observations on bees. Though I have had the good fortune to make some interesting discoveries, I am far from considering my labour finished. Several problems concerning the history of these insects still remain unsolved. Experiments which I now project may perhaps throw some light on them; and I shall be animated with much greater hopes of success, if you, Sir, will continue your counsels and direction. I am, with every sentiment of gratitude and respect,

FRANCIS HUBER.

Pregny, 1*st October* 1791.

PART II.

CHAPTER I.

SCHIRACH'S DISCOVERY CONFIRMED.

Perhaps it may seem extraordinary to recur to facts with which the reader has been already occupied, and those apparently corroborated by our own experiments. But the interests of truth call for the defence of a faithful observer, to whom our knowledge of bees is indebted for its greatest progress, and whose reputation has been so outrageously assailed.

Schirach began to undermine the opinion of Swammerdam, wherein both Maraldi and Reaumur concurred, that workers were not only sterile, but absolutely neuter. However, by repeated experiments, he proved that bees at all times can procure a queen to themselves, provided they have comb containing larvæ three days old in the common cells; whence he concludes, that workers are originally of the female sex, and that nothing but certain important conditions, such as a particular kind of food, and more spacious lodgement, are requisite for their conversion to queens.

Views so adverse to those generally entertained were received with enthusiasm, on the one hand, and with distrust on the other. Notwithstanding it was admitted, that bees might obtain a queen, because Schirach had shown this result from experiments scrupulously executed, and in the presence of intelligent persons it was maintained that the eggs had been laid in common cells; and the conversion of a worker's worm into a royal larva was denied. His experiments and his arguments were resumed in vain; the same objection was always urged against him, though observers, aided by the best microscopes, were unable to discern any difference among the larvæ, from which he could at pleasure produce either a common bee or a queen.

M. Bonnet, whose sanction, as that of a great naturalist, was anxiously desired by Schirach, requested me to repeat his experiments; and in doing so, I recognised the truth of the Lusatian observer's assertion. I added new illustrations, along with very convincing proofs of the disputed conversion; but I felt with him, that the establishment of such important facts rested on ascertaining the sex of the workers.

I reasoned thus:—Riem's discovery of fertile common bees, which has been confirmed by my own observations, leads me to anticipate that the whole class of workers belong to the female sex. Nature does nothing by leaps. Fertile workers lay none but male eggs—in this resembling queens whose fecundation has been retarded; advancing another step, they may remain absolutely sterile, while they are not the less females originally. Too many valuable properties, too much industry and activity belong to common bees, to permit my concluding them of monstrous conformation. Too many wonders result from their instinct and their structure, to permit my considering them the outcasts of the species, or imperfect beings when compared with queens.

Nothing is more repugnant to reason than an actual metamorphosis; and the theory of it once entertained is now explained by evolutions. If the queen has organs corresponding to those of the common bee, they are modified in such a manner as not to fulfil any of the same functions; and in saying the conversion of the one to the other is improbable, we shall be right. But in admitting the two insects to be of only one kind originally, and that their species is of the same individuality, we are justified in believing they may become either workers or queens. Some may imagine that a queen in the egg has become a worker from certain circumstances; others may equally affirm that the worker was originally there, from which certain modifications have formed a queen; for we cannot avoid thinking that the faculties and organs peculiar to the common bee pre-exist their evolution. We are thence bound to conclude, that this being, which as yet is neither worker nor queen—that the worm, before three days elapse, contains alike the germs of the insect which shall prove industrious, and of the insect susceptible of prodigious fecundity—the germs of the organs of the two animals, the instinct of the common bee, and that of the queen not developed, but capable of being so, according to the mode in which they shall be reared may operate. In the one case the productive faculties will be repressed, or will remain without being unfolded; in the other the industrial properties will continue in a similar condition.

Perhaps Nature presents beings of an intermediate kind between these two extremes, participating of the essence of queens, and of the qualities of workers; and hence the fertile common bees, and the small queens observed by Needham. It is easier to comprehend how certain faculties, and their corresponding organs, may be annihilated, than how they may be spontaneously created; and it is on this my theory is founded.

It may be opposed, indeed, by the opposite instincts of the workers and the queen in the same hive, in relation to other queens; for the workers entertain a kind of love for their mother, and render her the most assiduous attention, whereas queens are reciprocally animated by the most implacable animosity. But do we know how far any sensation may be evolved in insects by circumstances? Let me cite one example.

Very remarkable facts came under our notice in a republic of humble bees, where, as well as among domestic bees, there are three kinds of individuals. Several of the workers which, until a certain epoch, had lived in the best intelligence with the mother of the colony, having become fertile, they exhibited symptoms of the most violent jealousy. Some fell victims to the rage of others; and we saw the principal female perish by the stings of those which she had produced. Therefore, if such rivalship can arise among workers after participating of fecundation; if their affection for their companions and for their mother is convertible in an instant to hatred, the objection deducible from the different instinct of queens and workers, the strongest that may be urged against their primitive identity, is reduced to its proper estimate. After such a trait, who will deny that instinct is susceptible of change?

My speculations regarding the sex of workers at length received the most unexpected confirmation.

History of Black Bees.—In the year 1809 we remarked something particular in the treatment of certain bees by their companions at the entrance of a hive. On the 20th of June, a cluster in such irritation, that we durst not attempt to separate the bees, attracted our attention. Night approaching, prevented us from ascertaining the cause of this assemblage, but on the subsequent days, we frequently observed the bees occupied in defending the same hive against the entrance of some individuals whose external appearance was not absolutely similar to that of ordinary workers. Seizing several of them, we found that their difference consisted only in colour—that they were less downy on the thorax and abdomen, which gave them a blacker aspect, but as to the limbs, antennæ, body, and size, the whole external form presented perfect resemblance to common bees.

Their daily presence at the entrance of the hive plainly denoted that they were expelled from it by the workers; and in the combats between them, the latter always had the better. The common bee killed its adversary, or reducing it to such a state of weakness as to be incapable of resistance, carried it off in its teeth to a great distance from the hive.

We caught a number of the black bees, and introducing them into a vase, they speedily darted on each other, and were reciprocally killed. Some were confined in a glass vessel along with workers from the same hive; but no sooner had the latter observed them, than they were attacked and destroyed.

Every day the proscribed bees appeared more numerous. Once driven from the hive, they never returned, for when the sting spared them, they perished of hunger.

This singular scene continued during the whole remaining part of summer. Sometimes the black bees were not so cruelly treated by the workers, and then they seemed modified a little differently from the former—their animosities were abated, nor did they repeat their mutual encounters. But the rigour of the common bees was very soon resumed against them, and they were expelled anew.

We could not ascertain whether all the brood of this hive was attacked by that malady or particular state, rendering the black bees odious to their companions, and observing their number augmenting successively during some weeks, we had reason to believe that the whole offspring of the queen was affected. Black bees, however, were no longer seen in the end of September. The hive had apparently suffered from the exile of so many individuals—it was weaker than previously; yet we were encouraged as to the condition of the colony, when satisfied that the queen had not lost the faculty of laying eggs which produced workers perfect in every respect.

From the month of April in the following year this hive became the subject of observation; not a black bee appeared. The increase of workers was so great as to promise a swarm, but here we were disappointed;

therefore, our opinion was corroborated of the anomaly, whatever it might be, having affected only part of the eggs.

Was the queen entirely cured of this disposition to produce individuals of peculiar conformation? Was the vice hereditary, and what effect would it have on her female progeny?

It was proved that she had not been entirely cured without relapse; for in 1811, that is, two years after the bees originated, they re-appeared in great numbers under the same character and circumstances. During 1812 the hive threw a very fine swarm; and as the old queen always accompanies the new colony, we were not long of remarking the defective bees at the entrance of her habitation.

But it was more singular still to witness the same phenomenon in both hives at once. The old one had swarmed on the third of June, and on the second of July we perceived black bees at the entrance, which evidently could not belong to the brood of the queen that had passed into the new hive. Thus the progeny inherited the vice of the parent. We again observed the maltreatment of black bees at the entrance of the same two hives in 1813, but there were few of them.

Only a sketch of this subject is given here. Our object is to arouse the attention of naturalists, that a combination of facts may be obtained, for enlarging the history of imperfect bees.

I was anxious to investigate whether anything external or internal indicated the evolution of sexual organs in the proscribed insects; for I conceived that if they were real females, their presence might disquiet the common bees in respect to their queen, and perhaps their expulsion from the hive was to protect her from such rivals.

Dissection alone could verify the fact, but it required to be skilfully done. A young lady, Miss Jurine, who had devoted her time and the liberal gifts of Nature to similar studies, undertook this delicate task for me. Already did she rival Lyonet and Merian, yet we had soon to deplore her loss.

Miss Jurine, with her peculiar sagacity, proceeded to examine whether any difference from common workers was exhibited by the external organization of the black bees; but their identity seemed complete, excepting, as had occurred to ourselves, in a smaller quantity of down on the thorax and abdomen. However, when this expert naturalist extended her researches farther, and adopted suitable preparations, she discovered two distinct ovaries wherein no eggs were perceptible indeed, but analogous in form and substance to those of queens, though not so manifest to the observer. Plate II. fig. 8.

At first we concluded that a solution of the question had been obtained. We did not anticipate a more important discovery, which was to overturn our conjectures as to the persecution of the black bees, and which would unveil a mystery long obscuring the history of this tribe of insects.

A great number of common bees being taken indiscriminately from before the entrance of their hive, ovaries resembling those of the black bees were found in the body of each, after having been kept two days in brandy to darken the transparent membranes.

The value of this discovery must be felt by those who have followed the progress and the vicissitudes of the history of bees; and by those who have read the objections against Schirach's theory, which are founded exclusively on the absence of ovaries in workers. Thus, the belief of neuters among bees entirely vanishes, at the same time that the most interesting phenomena of their organization is exposed.*

The like should extend to all insects among which neuters have been described, that is humble bees, wasps, and ants; for, according to the observation of a great naturalist, the more important any organ is to the animal economy, the more general ought to be its existence.

We were not entitled to decide on the sex of the whole species from the fertility of individuals, until discovering the ovaries of working bees. But the concurrence of these two peculiarities being established, wherever one shall manifest itself under the same circumstances, the existence of the other may be presumed from analogy.

According to the observations of Riem, and in the former part of this work I think to have proved the fact, there are sometimes fertile workers in a hive, but never laying any except the eggs of males. I have shown that the cause to which we should ascribe their existence centres in a certain nutriment, resembling the sustenance of queens, operating so remarkable a change in their constitution. Their external characters are the same as those of the workers, but the

* M. Cuvier thinks he has observed minute chaplets in common bees resembling those in the oviducts of queens, which confirms our opinion of their being females, whose organization is not developed. *Lecons d'anatomie comparée*, tom. v. p. 198.

smallness of their number renders it almost impossible for us to observe the conduct of the latter towards them. Perhaps, by obtaining some in the frames used by Schirach for producing queens, and removing the royal cells at a suitable period, their habits, amidst a very few workers, might be studied. Then also would be the time to examine whether the fecundation of workers is attended by the same circumstances as that of queens.

Elsewhere we have explained the existence of fertile workers among humble bees, and described the jealousy awakened by maturity, their rivalship and resentments.* We have not recognised any other difference than size between the small mothers and real females, and having been then unable to ascertain whether the fertile workers had a progeny of both sexes, we lately engaged in researches to investigate the point.

A nest of red and black humble bees, the *hæmorhoidalis* of Linnæus, was established in an ordinary box in a window. The motion and agitation of the bees in the afternoon, their rivalship, and, finally, their laying, soon demonstrated that fertility was not restricted to the mother of the colony alone. But as the latter frequently laid her eggs in the same cells as the workers, the result of their fecundity could not be ascertained without an absolute separation, whereby the product of each might be known.

Therefore, a fragment containing nothing but brood was detached, and left in a box where the humble bees were accustomed to retire into their nest, and the mo-

* Transactions of the Linnæan Society, Vol. VI.

ther, together with the workers remaining at home, were carried with the other portion to a distant window. I calculated, on the return of the absent workers now foraging in the country, to people the portion wanting inhabitants. In fact they did so, and lodged themselves on the isolated fragment, which had been substituted for combs and brood, though they did not seem insensible of the change. I was in hopes also that some of these workers might prove fertile, nor were my prospects disappointed; for, in the afternoon of the same day of forming this separated or artificial swarm, they prepared a cell for receiving their eggs, and I saw many of them lay. The number of eggs multiplied daily—larvæ were soon hatched, and they transformed to nymphs, which, in a month, became humble bees. Having most carefully examined them as they proceeded from their cocoons, I found that all were males. They resembled those originating from the eggs of the female in every respect—they were equally large, and coloured after the same manner.

The red and black humble bee had been selected for this experiment, because bands of green hair on the thorax, and a green spot on the front, more easily distinguish the males than any others.

I could not be deceived regarding the fact, and I can affirm that neither worker nor female was produced in the one nest, while the other had as many females as males. Almost the whole workers were fertile in this nest except a few very small ones—at least I did not ascertain that these were so.

Here, therefore, is seen a great analogy between humble bees and the honey bee.

An additional example of the generality of the law of Nature against neuters was obtained by M. Perrot. Studying a hanging wasps' nest with that scrupulous attention which denotes the genuine naturalist, he observed some of the workers successively laying eggs. Although an accident prevented him from following out the complete evolution of the nymphs, careful inspection convinced him that all were of the male sex.

Ants likewise afford a striking analogy to what has been described. We never have seen the workers lay indeed, but we have witnessed their sexual union, as can be attested by several members of the Geneva Society of Natural History. The death of the workers always followed the approaches of the male, therefore, their conformation does not admit of their becoming mothers, but the instinct of the males proves them to be females.

All these facts concur in demonstrating that there are no real neuters in this class of insects which would have interrupted the chain of Nature, for I know not that it exists in any other kind. Sometimes both sexes are conjoined in one individual, but it seems adverse to Nature that there should be neuters.

How is it to be explained why the workers among a republic of insects, when fecundated, can lay none but the eggs of males? What reason shall we give for it? They have ovaries similar to those of the queen from which they have sprung, yet they possess only a partial fertility. It is equally difficult to understand why those queens impregnated later than three weeks after their origin produce the eggs of males alone. Doubtless some connection subsists between the two facts.

In attempting to explain Schirach's discovery, M.

Bonnet conjectures that the evolution of germs is effected by the action of the prolific matter as a stimulant, as a substantial nutriment suitable for that purpose; and he supposes that a certain quality of food, which is administered more copiously, may unfold those organs in the larvæ of bees that never would have appeared without it.

Is it impossible that this kind of subsistence, so different from what is received by common larvæ, being administered too late or too sparingly to the larvæ originating near the royal cells, may have consequences similar to the retarded fecundation of queens? Have not the fibres of the ovaries attained too great rigidity to be affected by the prolific matter on some of the eggs? Or does not the royal aliment, by an opposite operation, produce such an energy as to destroy this equilibrium?

In a work on the *Management of Bees in Favignana*, by Monticelli, a Neapolitan professor, Schirach is reproached with claiming the formation of artificial swarms as his invention, and with having borrowed it from the customs of a small colony inhabiting a rock in the Mediterranean, near the coast of Sicily. Schirach, however, was far from giving himself out as the author of a method practised long anterior to his own time in the country where he lived. Practice has always preceded theory—its success leads to discovery of the truths whereon it is founded, and acquaintance with these truths in its turn establishes the wavering advances of cultivators.

Schirach relates, that, being obliged to use a great quantity of smoke for driving bees to the top of their hive, in order that he might cut out the brood, they were so much annoyed by it as to depart in numbers unobserved, and accompanied by their queen. He sought the neighbourhood in vain for the fugitives; and abandoning the hive as lost for want of a queen, he resolved to rear a new one by introducing a comb consisting of three kinds of brood, such as that of which he had just deprived them. Next morning, preparing to clean the hives which he had divided on the preceding evening, he observed a cluster of bees the size of an apple, on the prop of the one whose queen had fled. Here he discovered a queen, and having carried her to the entrance of the hive which had lost its own, she was immediately surrounded by the bees, and treated in such a manner as plainly announced that she was their queen. "What was my astonishment," he proceeds, "when, wishing to introduce her among the combs, I saw that the bees remaining had already planned and almost finished three royal cells. Struck with the activity and the sagacity of these creatures to save themselves from impending destruction, I was filled with admiration, and adored the infinite goodness of God, in the care taken to perpetuate his works! Having carried away two of the cells to ascertain whether the bees would continue their operations, I beheld next morning, with the utmost surprise, that they had removed all the food from around the third worm left behind, on purpose to prevent its conversion to a queen."

Discovery of this kind of transformation soon rendered the art of making artificial swarms easier and less expensive. Schirach showed that its success depended on having a single cell occupied by a worm three days old, instead of providing the bees with large combs containing the three kinds of brood, as before supposed necessary. He proposed several improvements in procuring artificial swarms, but he never thought of claiming the invention.

The Italian author, wishing to gain credit to his country, and forgetting that the great republic of science is engaged rather with the utility and perfecting of discoveries than involved in disputes about their origin, openly charges Schirach with borrowing the plan of artificial swarms from Favignana. He affirms that the practice is of such ancient date, that even the Latin names are preserved by the inhabitants in their procedure; but he acknowledges at the same time, that both the Turks and Greeks of the islands in the Ionian Sea are acquainted with it. Two methods are followed—the one on a larger scale, more general and complete, which was unknown to Schirach, who imitates the other. The hives of the Favignanese are oblong wooden boxes, both ends of which are moveable. As spring is much earlier in their region than with us, they are able to commence their operations in the month of March. The hive, therefore, being transported to some distance from the apiary, the farther end is opened, and the bees are driven forward by means of smoke, when some of the combs commonly containing honey are cut out. Next they are driven back to the farther end, and a certain number of the combs are taken from the anterior part, some empty, and some full of brood of all ages. The latter are then put into a new hive, and, by means of rods inserted through them from without, they are established in the same order that they held in the old hive. After this is done, the new hive is conveyed to the station which was occupied by the old one, and the latter is removed fifty paces from the apiary. The bees now returning from the field, and finding a hive like that which they had left, lodge themselves there, rear the brood, and prosper.*

The success of this operation, which very much resembles Schirach's, amply confirms his theory; and we must reject Monticelli's prejudices, for his work is otherwise a good practical treatise on the management of bees, and the formation of artificial swarms.

We shall here take an opportunity of speaking of a very ingenious method, though somewhat different, followed by M. Lombard, a great cultivator of bees, and the author of an excellent treatise on their practical economy. It is the reverse of that adopted in Favignana to form an artificial swarm, for he obtains, so to speak, a natural precocious swarm. The hive destined for that purpose is carried to a dark place, already containing another, in which the new swarm is to be received. Part of the bees, together with their queen, are forced to take refuge in this new lodging by means of smoke; after which the old hive is replaced in its former situation, that it may be repeopled by all those returning from the fields, and the new one is established at a suitable distance from it in the apiary. This colony, possessing a queen, can prosper without any foreign aid, and enjoys the advantage of the flourish of the fruit trees, which is sometimes denied to the earliest natural swarms. The cylindrical form of M. Lombard's straw

* Probably these oblong boxes are laid horizontally on their props, and the entrance is made at the lower part of the end in front.—*T.*

hives facilitates the process. For the details I shall refer to his work itself, which is of essential utility to every cultivator of bees.

This method being founded on the production of a queen in a hive containing only brood, is a farther corroboration of Schirach's doctrine, for its long practice has been always attended with success.

Experiment and theory, therefore, concur in demonstrating that the larva of a bee may become a queen or a worker, according to circumstances. A female always appears, whether possessing the physical qualities of maternity as the fecundity of queens, or the conservative properties displayed by workers in regard for the young. This partition of industry and courage on the one hand, and of prodigious fertility on the other—this partition, originating from the mysterious rearing of the larvæ, is among the finest subjects for contemplation which natural history affords. Thus we owe one of the most curious discoveries which has embellished science to the penetration and perseverance of Schirach.

CHAPTER II.

ON THE SENSES OF BEES, AND PARTICULARLY THAT OF SMELLING.

The infinite varieties exhibited by the different tribes of insects and other animals excite our belief that the same impressions are not produced on their sensations as on man. Their faculties and nature not admitting the light of reason, they ought to be directed by other agents. Perhaps the opinions which we form of their senses, founded on those peculiar to ourselves, are incorrect, for some of a more subtle kind, or modified otherwise, may present objects under an aspect unknown to us, and occasion impressions that we are strangers to.

Cannot we admit that the same intelligence which dispenses to each animal the organization suitable to its tastes and habits, has the power of modifying its senses beyond any knowledge of them that we can acquire? Cannot the Author who framed those five grand avenues whereby all our notions of the physical world are gained, open up channels of another kind to beings less favoured with judgment? The assistance of art enables us to determine regarding objects not so immediately within the sphere of the senses, as chemistry and physics prove in a thousand examples. Thermometers, menstrua, and re-agents, by whose means we detect the nature of things eluding our senses, are so many new organs. Thus there may be other sensations and means of viewing objects, and it is not repugnant to admit the existence of such in beings so differently constituted from us in many respects.

Insects living in a republic, among which bees certainly occupy the highest rank, often exhibit traits inexplicable, even admitting them to be endowed with the same senses as ourselves, which renders it so difficult to penetrate the secret impulse actuating them. Yet they possess sensations of a less subtile description, and, as it is expedient to gain as much knowledge of their powers as possible, we should be wrong to neglect what comes within our reach, and whereby we can judge at least of their appetites and aversions.

Sight, feeling, smell, and taste, are the senses commonly ascribed to bees; for hitherto we have no proof that they enjoy the sense of hearing, although a custom very prevalent in the country seems to support a contrary opinion—I allude to the practice of striking a sonorous body at the moment of departure, in order to restrain a swarm from flying away. But, in compensation, how great is the perfection of their organ of sight! Since from a distance the bee recognises its habitation amidst an apiary of numerous others resembling it, and returns in a straight line with great velocity, we must suppose that it is distinguished by marks escaping our notice. The bee departs and flies straight to the most flowery field. Having ascertained its course, it is seen traversing it as directly as the flight of a cannon or a musket ball. Its collection being made, it rises aloft in the air to reconnoitre its hive, and returns with the rapidity of lightning.*

The sense of feeling is still more admirable—it is substituted for, and completely supplies the want of sight in the interior of the hive. The bee constructs its combs in darkness, it pours its honey into the magazines, feeds its young, judges of their age and necessities, recognizes its queen, all by means of the antennæ, which are much less adapted for becoming acquainted with objects than our hand. Therefore, shall we not grant to this sense modifications and perfections unknown to the touch of man?

Perhaps the sense of taste is the least perfect of those enjoyed by bees, for this is one which in general seems to admit of selection. But, contrary to the received opinion, the bee certainly displays little choice in the honey that it collects. It is not repulsed by plants which to us are the most hateful and odious—poisonous flowers are not excluded from its search, and the honey of some of the American provinces is even said to be of the most deleterious nature. Neither do bees despise the secretions of aphides, notwithstanding of so impure an origin; nor

* The sense of vision among insects seems of an obscure complicated nature, and that of the tribes of mollusca and vermes is still more indefinite. Naturalists generally believe that in the former it resides both in the large reticulated or granulated eyes on each side of the head, and in the three ocelli or specks on its upper part. It is remarkable that the eyes of bees are covered with hair. These insects, blinded by an opaque varnish, fly at random, or rise vertically, until disappearing in the air.—*T.*

do they testify greater nicety in the quality of their water, for the most corrupted marshes and ditches seem preferred to the most limpid streams, nay, to dew itself.

Nothing, therefore, is more unequal than the quality of honey. The produce of one district differs from that of another—the honey of spring is unlike that of autumn; nor do the contents of a certain hive always resemble those of the hive adjoining.

If bees have little choice in their nutriment, and are not delicate in regard to the quality of honey, they are far from testifying the same indifference as to the quantity contained in flowers—they constantly resort where most is to be found. They quit their hive much less in regard to the fineness or temperature of the weather, than according to their prospects of a more plentiful or scanty collection. When the lime-tree and black grain blossom, they brave the rain, they depart before sunrise, and return later than ordinary. But this activity relaxes after the flowers have faded; and when the enamel adorning the meadow has fallen under the scythe, the bees remain in their dwelling, however brilliant the sunshine. To what shall we ascribe their perceptions of the abundance or deficiency of honey in the flowers, which the whole colony appears to possess without leaving their habitation? Does one sense more subtle than the others, namely smelling, warn them of it?

There are some odours, such as the smoke of tobacco, and every kind of smoke, to which bees are averse, and some that are agreeable to them; but of all odorous substances, honey is that which has the most powerful attraction. Perhaps others have a similar influence, only in as far as announcing the presence of what seems so valuable.

To ascertain whether the appearance of the flowers or the odour of the honey apprises bees of its presence, we placed honey in a window, near a hive, where the shutters, almost close, still allowed their passage if they chose. Within a quarter of an hour four bees and a butterfly had insinuated themselves, and we found them feeding on it. Although this was sufficiently conclusive of my opinion, I wished it better confirmed; therefore I had four boxes, different in size, shape, and colour, made, with small card valves, corresponding to apertures in the covering. Honey being put into them, they were placed at the distance of two hundred paces from my apiary. In half an hour bees were seen arriving; they carefully traversed the boxes, and soon discovering the openings whereby they might introduce themselves, they pressed against the valves and reached the honey.

We may thence judge of the extreme delicacy of smelling in these insects. Not only was the honey quite concealed from view, but its emanations, from it being covered and disguised in the experiment, could not be much diffused.

Flowers frequently exhibit an organization resembling that of the valves we allude to. The nectarium of several classes is situated at the bottom of a tube, inclosed or concealed among the petals, nevertheless the bee finds it out. Still its instinct, less refined than that of the humble bee, affords fewer resources. The latter, when unable to penetrate the flowers by their natural cavity, knows to make an aperture at the base of the corolla, or even of the calyx, and to insinuate its proboscis to the place where Nature has situated the reservoir of honey. By means of this stratagem, and favoured by the length of its organ, the humble bee can obtain honey where the common bee would reach it with great difficulty. From the difference of the honey produced by these two insects, we may conjecture that their collections are not made from the same flowers. Common bees, however, are as much attracted by the honey of the humble bee as by their own. We have seen them pillage one of their nests in time of scarcity, which had been placed in an open box near an apiary, and carry the contents almost totally away. Some of the humble bees remaining after this disaster, still repaired to the fields, and returned with the surplus of their provender to their ancient asylum. But the plunderers, posting in pursuit, accompanied them home, and never quitted them until having obtained the fruits of their labour. They licked the others, offered their trunks, surrounded them, and departed only after they had rendered up the saccharine fluid of which they were the depositories. No injury was done to the insect which thus afforded the bees their repast—the sting never was unsheathed, and the humble bee itself, accustomed to such exactions, yielded up its honey and resumed its flight. This remarkable kind of intercourse lasted above three weeks. Wasps, invited by the same attraction, did not become so familiar with the original proprietors of the nest. At night it was occupied by the humble bees alone. At length they disappeared, nor did the parasite insects return.

We have been assured that the same scene may be witnessed between predatory bees and those of weak hives, which is less suprising.

Not only have these insects a very acute sense of smelling, but to this property is added the recollection of sensations. Honey had been placed in a window in autumn, where the bees resorted to it in multitudes. It was removed, and the shutters closed during winter; but when opened again, on the return of spring, the bees came back, though no honey was there. Undoubtedly they remembered it, therefore an interval of several weeks did not obliterate the impression they had received.

Let us now inquire into the site or the organ of this sense, whose existence has been so well established.

Nostrils have not been yet recognised in insects, nor do we know in what part of the body they, or any organs corresponding to them, are placed. Probably odours reach the sensorium through the medium of a mechanism similar to our own—that is, the air is introduced into some opening at the termination of the olfactory nerves, whence we should examine if the stigmata do not perform this function, or whether the organ we are in quest of be not situated in the head, or in some other part of the body.* With the view of elucidating the matter, we made the following experiments :—

1. A pencil dipped in oil of turpentine, one of the substances most formidable to insects, was presented

* Certain apertures, generally called stigmata, appear on each side of the body of insects, which naturalists believe to be appropriated exclusively for respiration.—*T.*

successively to all parts of the body of a bee, which did not appear in the least affected, whether on approaching the thorax, abdomen, or stigmata of the thorax.

2. We then took a very fine pencil, that it might reach every point of the head, and brought it near the antennæ, the eyes, and protruded trunk of a bee, in the act of feeding, but in vain. It was otherwise on carrying it near the cavity of the mouth, above the insertion of the proboscis. At that instant the bee receded, left the honey, and beating its wings, while moving about in much agitation, it would have taken flight had not the pencil been withdrawn. Having renewed its repast, we resumed the application, always carrying the impregnated portion near the mouth. The bee now quitting the honey, fixed on the table, and fanned itself during some minutes. The organ of smelling, therefore, seems to reside in the mouth itself, or in the parts depending on it.

Bees not occupied in feeding appeared more sensible of the odour of the turpentine. They were affected by it at a greater distance, and speedily took flight, whereas, when so engaged with the trunk immersed in honey, several parts of the body might be touched by the pencil without their withdrawing. Were they absorbed by their greediness of the honey, and distracted by its smell, or were their organs less exposed? This could be ascertained in two ways, either by covering all parts of the body with a varnish, and leaving the sensible organ free, or leaving the whole parts untouched excepting that wherein the sense was supposed to reside.

The latter method appearing the more practicable and decisive, we seized several bees and compelling them to unfold the trunk, filled the mouth with flour paste. When this obstruction was dry enough to prevent them from divesting themselves of it, they were released—none seemed to suffer any inconvenience. They breathed and moved with the same facility as their companions. Honey did not attract them—they neither approached it, nor were they affected, apparently, by odours otherwise offensive to them. Pencils were dipped in the oil of turpentine and cloves, in ether, in fixed and volatile alkalis, and their points insinuated very near the mouth; but the odour of these fluids, which would have occasioned a sudden shock to bees in their natural state, had no sensible effect on them. On the contrary, several mounted on the impregnated pencils, and traversed them as if unhurt; therefore, they had at once lost the sense of smelling, the site of which it appeared to us resided in the mouth.

In the next place we wished to investigate how bees were affected by different odours.

Mineral acids and volatile alkali presented on a pencil to the opening of the mouth, made the same impression as the spirit of turpentine, but acted with greater energy. The effect of some other substances was not so decided. On our presenting musk to bees feeding before the entrance of their hive they ceased, and dispersed a little, but without precipitation or beating their wings. We threw it pulverized on a drop of honey, into which the bees thrust their trunks, but as if by stealth, and keeping at as great a distance as possible. Had the drop not been covered with musk it would have disappeared in a few minutes, now it had suffered no sensible diminution in a quarter of an hour, although the bees had often plunged their trunks amongst it.

Experiments were made with substances which do not undergo any perceptible alteration; for certain odours vitiate the air without directly affecting the nerves.

3. Pounded assa-fœtida, whose odour is insupportable to us, being mixed with honey, and put at the entrance of a hive, did not seem to annoy the bees. They greedily absorbed all the honey in contact with the extraneous particles—they neither attempted to withdraw nor vibrated their wings, and of the mixture only the molecules of assa-fœtida at last remained.

4. I remarked that bees going out to the fields, and coming home, turned aside in the air to avoid passing immediately over a piece of camphor laid before the entrance of their hive. Some being attracted by honey on a card, I brought the camphor towards the mouth, while their trunks were immersed among it. All took flight, but after flying about some time in my cabinet they settled beside the honey. While absorbing it, I threw fragments of camphor on the surface; they receded a little, still keeping the end of the trunk inserted, and we observed that at first they took only that which was not covered with the camphor. One vibrated its wings as it fed, some agitated them seldom, and others not at all. I covered the honey entirely with camphor, and at that instant all flew away. This card was carried to my hives, and pure honey also put within reach of the bees on another. The latter was soon discovered, and the honey consumed in a few minutes; but an hour

elapsed before a single worker approached the camphorated card, when two resting on it, thrust their trunks into the edge of the drop of honey. Their number gradually augmented, and two hours after it was covered by them, the whole carried off, and only the camphor remained.

These experiments prove, that if camphor be disagreeable to bees, their repugnance is surmounted by the attraction of honey, and that there are odours which repel them to a certain degree without vitiating the air.

It is shown by many experiments also, that the influence of odours on the nervous system of bees is incomparably more active in a close vessel than in the open air.

5. A bee having been satiated with honey was put under a receiver containing a small glass of spirit of wine, allowed to evaporate. Immediately endeavouring to escape, it incessantly vibrated its wings and sought an exit for an hour. A constant tremor of the limbs, wings, and proboscis, then became perceptible—the insect, unable to stand, lay supine, and in this position advanced in a strange manner, using the four wings like oars or feet, and all the honey swallowed before exposure to the effluvia of the spirit of wine was disgorged. Immersion twice in cold water was beneficial to the bee, and vinegar seemed to revive it, but it soon perished.

Window flies and wood lice were destroyed by the same effluvia, however, a large spider did not appear to be affected.

6. It was curious to ascertain the impression that the penetrating odour of the poison of bees would have

on the animals themselves, therefore, the sting of one, with its numerous appendages, being extracted with pincers, was presented to workers in a tranquil state, before the entrance of their dwelling. Emotion instantly spread among them—none fled away, but two or three darted against the sting, and one wrathfully attacked ourselves. It was not the threatening apparatus of the experiment, however, which had irritated them, for, when the poison had coagulated on the point of the sting, and on its appendages, they seemed insensible of its presence, and it might be offered with impunity. It is the odour of the poison alone that enrages them. Having put some bees into a glass tube, close at only one end, we rendered them half torpid, in order to prevent their escape by the other. They were gradually revived by the heat of the sun, and then irritated with the beard of an ear of barley. All protruded the sting, on the extremity of which appeared drops of poison.

Their first symptoms of life, therefore, were attended by demonstrations of anger; and I doubt not, had they been at liberty, they would have pierced each other or flown on the observer. But they were retained in the tube.

I took them out one by one with pincers, and confined them in a receiver, that my experiment might not be disturbed. The poison darted against the inside of the tube had left an unpleasant smell. When the bees became sensible of this, from my presenting the open extremity to groups before their hive, they were agitated, but their agitation was not the result of fear, for they testified anger in the same way on as the former trial.

Thus, certain odours not only act physically, but have an influence on the inclinations of these insects: and here there doubtless commences a particular class of sensations eluding our researches, and of which we can form but a confused conception. What a variety of impressions are produced by smell on dogs of the chase!

In accounting for the conduct of insects under special circumstances, the influence of different sensations, modifying their habits at the moment, must be appreciated.

Bees are impelled to flight by too high a temperature, or repulsive odours. But if invited by some other cause, as the attraction of honey, to remain, they know to preserve their present enjoyments, and shelter themselves from disagreeable sensations, by agitating the air. Retained in their hive by all the attractions which Nature has there combined for them, and unable to withdraw from the surrounding mephitic gas, without abandoning their young together with their stores, they have recourse to ventilation for renewal of the air. To what can we ascribe the general tranquillity, when a few individuals are occupied in obtaining a salubrious atmosphere? We cannot suppose only a portion of them affected by a cause which has no operation on the greater number. When the air was not renewed in the manner desirable, however, we have seen all vibrating their wings at once, though this never occurs in the natural state, when the vibrations of a few are sufficient for ventilation.

Some insects of the same species are more immediately affected than others by the same cause, which may ensue from circumstances or their occupations.

Therefore, after the air has been rendered of sufficient salubrity by a certain number of ventilators, the rest, not experiencing the sensation of its impurity to an equal extent, abstain from vibrating their wings, and yield to more urgent pursuits. Should the number of ventilating bees be suddenly reduced, the workers first experiencing the alteration of the air will begin to vibrate, and their number will augment, until their united exertions restore to this element the degree of purity essential to respiration.

Such we conceive is the mode of establishing the perpetual series of ventilating bees, for no communication is observed between them.

This hypothesis presumes an organization sufficiently delicate to detect the smallest alteration of the respired fluid, which may lose many degrees of its purity before we are aware of it, though proving very noxious to us.

CHAPTER III.

RESEARCHES ON THE USE OF THE ANTENNÆ IN SOME COMPLICATED OPERATIONS OF BEES.

We have examined the general relation of the senses of bees to objects of immediate utility. But it is extremely probable that the sphere of their activity is not restricted to distinguishing odours and the substances these insects have to collect. The art of procuring their materials and putting them in operation, is only one branch of the history of bees. Their conduct as a great society, whose prosperity depends on variable elements, should offer civil connections, so to speak, between all its members.

As their senses undoubtedly must have a large share in the operations resulting from this state of things, it is important to inquire what influence should be ascribed to them when instinct seems adapted to the most complicated circumstances.

One of the facts which appears the most deserving of meditation and research, is the formation of a queen in a hive that has lost its own. Whether the worker be confident or not of attaining its purpose, by changing the food and form of the cell destined for a royal worm, its conduct certainly displays a refinement of instinct, of which we could not believe an insect capable.

In given circumstances, though very rarely, the colony runs the hazard of destruction by losing its queen. The precautions of the bees avert this disaster. But how are they invited to it? How can the absence of the queen guide them to so complex and remarkable a proceeding, as choosing larvæ of that age which will admit of what is to be accomplished?

Did the absence of the queen alone produce these effects, we should observe new cells constructed immediately after her disappearance. But the bees do not seem to miss her; the works of every kind advance; order and tranquillity are uninterrupted; nor is it until an hour from her departure that disquiet testifies itself among the workers, when they hurriedly traverse the hive, and are no longer engrossed by care of the young. Yet the first symptoms of agitation are not felt every where at once. They originate on a single portion of the combs—the disturbed bees soon quit their little circle, and meeting their companions, the antennæ are reciprocally crossed, and they slightly strike them. Those receiving the blow of the antennæ become agitated in their turn, and carry trouble and confusion to other places—the disorder augmenting in rapid progression, gains the opposite side of the comb, and is at length disseminated throughout the colony. Then are the workers seen running over the combs, rushing against each other, and impetuously hurrying towards the outlets of the hive. From thence they disperse themselves around. They return and depart repeatedly; the buzzing is very great within, and it increases with the disorder of the bees. This continues, however, only two or three hours, seldom four or five, but never longer.

What is the source of such an effervescence? How do the bees gradually recover their natural state, and resume an interest in all that seemed to have become indifferent to them? How are they recalled spontaneously to their young, which for some hours they had abandoned? And whence is their inducement afterwards to visit the larvæ of different ages, to select from among them some to be reared as queens?

In twenty-four hours subsequent to the departure of the common mother, we see that the bees have been labouring to repair her loss. The cells occupied by the favoured larvæ are always of the smallest diameter; their form has not yet been altered, but they are already distinguished by the quantity of paste in them, which infinitely exceeds the portion devoted to the larvæ of workers. It follows that, by this profusion, instead of being lodged at the bottom of the cells, they are now brought very near the orifice. Probably it is to give these larvæ an elevated position that the bees accumulate the paste behind them; and it is proved not to be for food, by our finding it entire when the worm has descended into the pyramidal prolongation by which the workers terminate their abode.

We, therefore, know what larvæ are destined for queens by the aspect of the cells occupied by them, even previous to their enlargement and alteration of shape, and by attending to this, we discover easily, at the end of twenty-four hours, whether the bees have resolved to repair the loss of their queen.

In whatever manner they ascertain her removal, it is evidently known to all within an hour; they feel themselves in a distressing condition, and seek for the object of their solicitude. It may be asked, Whether it is by means of smell, touch, or any unknown sense, or whether from the bees communicating with each other by representative signs? Experiment might disclose the fact.

I divided a hive into two portions by means of a grating, executing the operation with such expedition and delicacy, that the smallest agitation was imperceptible, nor was a single bee injured. The bars of the grating admitted the free circulation of air, but were too close for the reciprocal passage of the bees. I did not know which half contained the queen, but the tumult and buzzing in No. 1 soon assured me that she was in the other, No. 2, where quietness and tranquillity prevailed. Still preserving the circulation of the air, I closed the entrance of both, that the bees, seeking for their queen, should not find her. In two hours they calmed, and the wonted order was restored in all respects.

On the fourteenth (day of the month that this experiment was made) we saw the commencement of three royal cells in No. 1. The entrance of both halves was opened on the fifteenth; the bees went out to forage, and on their return we observed that they kept to their respective hives without intermixing. Two queens were observed dead at the entrance of No. 1 on the twenty-fourth; and, in examining the combs, we found her that had killed them. She left the hive on the thirtieth, was fecundated, and from thenceforward the prosperity of the colony was ensured.

The apertures in the division between the halves allowed the communication of the bees of No. 1 with this queen, by means of smelling, hearing, or any unknown sense. They were separated by an interval not exceeding a third or a fourth part of an inch, which they could not pass; yet the same bees became agitated; they constructed royal cells, and reared young queens;—therefore they conducted themselves just as if their queen had been truly carried away, and lost to them for ever.

This observation proves that it was neither from sight, hearing, nor smell, that the bees were sensible of the presence of their queen, and that the aid of another sense was interposed. The division inserted between the halves of the hive having deprived them of nothing but contact with her, was it not very probable that her presence had to be learned by touching her with the antennæ? It is by means of these organs that bees gain the knowledge of their combs, their young, their companions, and also of their queen, all communicated by the sense of feeling.

To be satisfied on the point, a frame was removed from one of my glass hives, and replaced by a box of the same dimensions, covered within by a grating, which allowed transmission of the antennæ, but too small for the heads of the bees. A moveable glass frame closed the other side.

Waiting until the queen placed herself on the front of a comb in view, we opened the frame, and took her from amidst her companions without alarming them. She was immediately confined in the glass box destined to receive her; but that she might not suffer too much from a situation so different from what she had been accustomed to, some bees of the same hive were introduced along with her, from which she experienced the wonted attentions.

We remarked from the first, that the distress commonly following the departure of a queen was not manifested on this occasion. Every thing remained in order ; the bees did not forsake their brood during a single moment ; their labours were uninterrupted ; nor did we see the rudiments of any royal cells forty-eight hours afterwards on opening the hive. No arrangements had been made for obtaining another queen. Thus, all the bees knew that they had no need to replace her, that she was not lost ; neither did they treat her as a stranger on being restored, they seemed to recognise her immediately. She soon began to lay amidst the surrounding circle of workers.

The means of communicating with this queen, which the bees adopted during her seclusion, were very singular. An infinite number of antennæ thrust through the grating, and turning in all directions, plainly indicated that they were occupied with her. She acknowledged the interest which was taken in the most decided manner, by always remaining fixed on the grating, and crossing her antennæ with those so evidently employed in ascertaining her presence. The bees attempted to pull her through to themselves, for her legs were seized and firmly held by theirs passing to the other side. Their trunks, likewise, were observed to be introduced to the queen's division ; and while a captive, she was fed by her subjects from within the hive.

How can we doubt, after this experiment, that the workers and the queen preserved a communication by the mutual touch of the antennæ; but knowing she was so near, the latter felt that there was no necessity for providing themselves with another.

To prove that it is not by smell that bees are sensible of her presence, I put the queen of one of my leaf hives into a box with a double grating, too distant for the operation of the antennæ. We had foreseen the result. After remaining calm an hour, the bees became agitated; they abandoned their labours, and forsook their young; left the hive, and returned; but in two or three hours tranquillity was restored. Next morning we recognised the rudiments of eight or ten royal cells, which had been commenced since the preceding evening. This proved to demonstration that the bees had believed their queen lost, though she resided among them. Her emanations, therefore, were not sufficient to undeceive them, and they required actual contact with her to be assured of her presence. As every bee cannot be in all parts of the hive, we must admit likewise that they mutually communicate their disquiet, and that they labour in common to repair their loss.

Perhaps the experiments made on the antennæ of the queen will be recollected:—That the amputation of one did not affect her instinct, but if both were cut off near the root, that these beings so privileged, these mothers so much the object of consideration, lost all their influence—even the instinct of maternity disappeared; their eggs were not deposited in cells; their mutual animosities also were forgot; they passed close together without recognising each other, and the workers themselves seemed to participate in their difference, as if nothing except the agitation of the queen rendered them aware of danger to their colony.

We amputated the antennæ of two hundred males and three hundred workers. The latter being releas-ed, speedily revisited the hive; but we observed that they did not ascend the combs as usual, that they no longer shared in the common operations, and pertinaciously remained below where some light was admitted from the entrance. This proved their sole attraction. They soon departed to return no more.

The same effects were produced on the males. They likewise retreated to their habitations, but now they could not find the internal passages, and they rushed towards the side where an open shutter admitted the light, there finding an exit. Some were seen requiring honey from the workers, but in vain; they could no longer guide the proboscis; it was directed blindly towards the head or the thorax, therefore they obtained no relief. On excluding the light, they hurried out of the hive though it was six o'clock, when no males left the others. Thus their departure must be ascribed to the loss of that sense which is employed to guide them in the dark.

As the amputation of only one of the antennæ had no perceptible effect on the instinct of queens, neither had a similar mutilation any influence on that of males or workers. Privation of a small portion of these organs did not impair their faculty of recognising objects, for they remained in the hive and pursued their wonted labours. Therefore, the conduct of bees deprived of the antennæ cannot be ascribed to pain; it ensues from their incapacity to guide themselves and communicate with other members of the colony.

My conjectures are supported by the use to which bees, particularly in the night, apply the antennæ. Let us follow their operations by moon-shine, when they keep watch at the opening of the hive, to prevent the intrusion of moths then on the wing, and we shall be convinced of the fact. It is curious to observe how artfully the moth knows to profit to the disadvantage of the bees, which require much light for seeing objects, and the precautions taken by the latter in reconnoitring and expelling so dangerous an enemy. Like vigilant centinels they patrole around their habitation, with their antennæ stretched out straight before them, or turning to right and left—wo to the moth if it cannot escape their contact. It tries to glide along between the guards, carefully avoiding their flexible organs, as if aware that its safety depended on its caution.

We have not affirmed that bees possess the sense of hearing; yet we shall avow that we have been tempted to believe it so. Frequently we hear an abrupt agitated sound among those watching the entrance of the hive during the night; but if a stranger insect, or any enemy happens to touch their antennæ, the guard is roused, the sound assumes a different character from that produced by the bees while buzzing or in flight, and the enemy is assailed by several workers from the interior.

Should we strike the board of a hive, the bees soon put their wings in motion; but on breathing through a cleft of their habitation, we hear some of them, by means of their wings, produce sharp and interrupted sounds; and then other workers are observed in agitation, and directing themselves from the side where the air has entered.

These facts seem to correspond with the sound emitted by queens, in leading us to admit a sense in bees analagous to hearing. Yet it must be remarked that they receive no impression from sounds which are not relative to their instinct. They do not seem to be affected by thunder or the report of fire-arms. Thus, if the sense of hearing does subsist among them, it is differently modified from the same sense among beings of a higher order.

We shall restrict ourselves to observing, that certain sounds produced by bees apparently serve as a signal to their companions, and are followed by regular consequences. They may be additional means of communication to those afforded by the antennæ.

There is nothing repugnant to our conceiving the subsistence of a language among beings whose instinct is so enlarged as that of bees, whose conduct is compounded of a thousand proceedings, and which, combined in great numbers, cannot share their respective labours, or aid each other suitably, without the means of mutual communication. The like observation applies to all insects dwelling in society, as well as those large animals regulated by the same conditions.

CHAPTER IV.

ON THE RESPIRATION OF BEES.

The respiration of insects accumulated together in a confined space, where the air can be renewed with difficulty, offers a new problem to the naturalist. Such is the case with regard to bees. Their hive, whose dimensions do not exceed one or two cubic feet, contains a multitude of individuals, all animated, active, and laborious. Its entrance, which is constantly very restricted, and often obstructed, by crowds of bees departing and arriving during the heats of summer, is the only opening admitting the air, yet it suffices for their exigencies. The hive, besides being internally plastered over with wax and propolis by its inhabitants themselves, and closed up with lime from without by the cultivator, has none of the conditions necessary for preserving a current of natural air.

If a lighted taper be placed in a vessel of equal capacity as a hive with an aperture larger in proportion than its entrance, the flame grows pale in a few minutes, then burns bluish, and is extinguished. Animals in the same situation would soon expire, yet how do the bees survive in their dwelling?

ARTICLE I. *Proofs of Respiration.*—Experiment 1. Bees in the receiver of an air pump were not affected by the first strokes of the piston; but when the mercury sunk to a quarter of an inch above the level of the cistern, they fell down motionless. Exposure to the open air revived them.

2. I took three empty flagons, each capable of holding 16 ounces of water, and introduced 250 workers into the first, the same number into the second, and 150 males into the third. The first and last were shut close, the second only restrained the escape of the bees, that they might serve for comparison. In a quarter of an hour the workers in the close vessel began to testify signs of uneasiness; their rings contracted and dilated with greater rapidity; they perspired copiously and seemed strongly affected, because they licked the humid sides of their vessel. In another quarter of an hour, a cluster, which had formed around a bit of straw, suddenly separated, and each of the bees fell to the bottom of the vessel, incapable of rising. All became asphyxiated in three quarters of an hour; nevertheless, when removed and exposed to the air, they recovered. The males were affected more fatally, for none survived, but the bees in the vessel admitting the air did not suffer. We found the air of the others greatly altered—the oxygen gas was almost totally consumed, and bees now introduced into it perished.

3, 4. Experiment demonstrated, that a new supply of oxygen gas restored the asphyxiated bees. When this gas was pure, some of them put among it lived eight times as long as in common air, but all were suffocated by its conversion into carbonic acid gas.

5, 6, 7. Bees became insensible instantly in carbonic acid gas obtained from chalk, though they quickly revived in the open air: but they perished instantly and irrecoverably in that obtained from a mixture of sulphur and iron filings, and the same succeeded in hydrogen gas obtained from zinc.

8, 9. Bees had expired in an hour in a mixture of three parts of hydrogen gas and one of vital air, the whole volume of both equalling that of six ounces of water. They perished as instantaneously in an atmosphere composed of three parts of azotic gas from sulphur and iron filings moistened, and one of air.

10. To ascertain the effect of vitiated air on these insects in a torpid state, we surrounded a receiver containing them with pounded ice. The thermometer fell from 65 deg., the temperature of the atmosphere, to 45 deg., when their torpidity commenced. Being transferred into tubes full of the gas, which had been so fatal to those of the preceding experiment, and kept among it three hours, they revived afterwards by the heat of the hand. It is not contact of the mephitic gas, therefore, but its introduction into the respiratory organs, that is fatal.

11, 12, 13. The result of experiments on the eggs, larvæ, and nymphs of bees was similar, proving the consumption of oxygen and the formation of carbonic acid gas. The larvæ consumed more than the eggs, and less than the nymphs, but the last alone perished.

Larvæ resisted the pernicious influence of carbonic acid gas better, during some seconds, than adult bees would have done. Nymphs died almost instantly.

14. Eggs put in air vitiated by the respiration of bees, lost the faculty of evolution ; but larvæ and nymphs rendered torpid endured exposure to deleterious gases several hours without inconvenience.

The respiration of bees at an early age was thus proved to be regulated by the same laws as that of adults, as was to be expected from Swammerdam having observed three pair of stigmata on the thorax and seven on the abdomen of nymphs. But I consider it of some importance to ascertain whether these organs are preserved in the adult insect. The wonted method of immersion was employed; and to avoid any complication which might ensue from torpidity, the water was slightly heated.

15, 16, 17. When only the head of a bee was plunged in mercury, or in water, it did not seem to suffer, but if the head alone remained out of the fluid the insect displayed its trunk, and soon became asphyxiated; if the head and thorax were immersed, leaving the abdomen free, the bee struggled a few seconds and quickly died.

18. The orifices of the respiratory organs being apparently in the thorax, it was left free, while the head and abdomen were immersed. A bee supported the experiment very patiently, and took flight when released.

19, 20. The action of the stigmata can be best observed by the suffocation of bees in water. Four air bubbles then become conspicuous—two between the origin of the neck and the root of the wings, a third on the neck at the origin of the proboscis and the fourth on the opposite extremity of the thorax, close to the pedicle uniting it with the abdomen. The bee seems to have some power in their retention, as they do not rise to the surface of the water, until acquiring sufficient size to overcome the resistance of inspiration, or adherence to the sides of the cavities. By the last two bubbles, the existence of stigmata, which Swammerdam overlooked, is indicated. As other experiments showed that one orifice remaining free is sufficient for carrying on respiration, some internal communication must subsist between the stigmata.

21. We ascertained from the precipitation of lime from lime water by air bubbles escaping from the body of bees, that the formation of the carbonic acid gas is chiefly owing to their respiration.

Article II. *On the air of hives.*—Experiment 1. It could not be supposed that the atmosphere surrounding 25,000 or 30,000 bees, or sometimes more, preserved a sufficient degree of purity for their respiration. Yet, to prove it, we prepared a large tubular receiver for a hive; and after lodging a swarm there, allowed time for constructing some combs, that the condition of the bees might be assimilated to their ordinary state. In one experiment, the air of the receiver, taken at different times of the day, proved nearly as pure as atmospheric air, and equally so in another. We found, besides, that wax and pollen rather vitiated its purity.

2. If the bees had any sources of vital air in their hive, capable of supporting their necessities, it would be indifferent whether the entrance were close or open.

Therefore, while great activity prevailed, and their buzzing in the receiver was audible at the distance of ten paces, I shut up the entrance at three o'clock on a rainy day, when the whole bees were collected within. In a quarter of an hour they began to testify some uneasiness, for, until that time, they seemed unconscious of their imprisonment. But their labours were now suspended, and the hive assumed quite a different aspect—all the bees, those covering the surface of the combs, as well as those clustering together, quitted their stations, and vibrated their wings in great agitation. This ferment continued about ten minutes, when the motion of the wings gradually relaxed, and became less incessant. At 37 minutes past three, the workers had lost their strength—they could not cling by the legs, and speedily fell down. The number of falling bees always augmented, until thousands strewed the board of the hive. Not one remained in the combs, and three minutes later the whole colony was suffocated. The hive cooled suddenly, the thermometer in it sinking from 95 degrees to the temperature of the atmosphere. In hopes that the admission of purer air would restore heat and animation, we opened a stopcock adapted to the hive, and also its entrance. The effects of the current introduced were unequivocal. In a few minutes the bees were in a condition to respire, the rings of the abdomen began to play, the vibration of their wings commenced simultaneously—a very remarkable fact, which we had previously remarked at the moment privation of the external air had been felt. The bees in a short time re-ascended their combs, the temperature rose to the degree which these insects know to preserve habitually, and in a few hours order was established in their dwelling.

Undoubted evidence was thus obtained that bees have no substitute in their hive for external air.

ARTICLE III. *Researches on the mode of renewing the air in hives.*—While investigating all the faculties of bees themselves, which might effect renewal of the internal air, we were struck with the vibration of the wings. We suspected that, having sufficient action to produce the continual buzzing heard within, it might be for the purpose of displacing the air vitiated by respiration.

During fine weather, a certain number of bees always appear before the entrance of the hive occupied in this manner, but still more are found to be engaged in ventilating the interior. The ordinary place of ventilation is on the board—those outside of the entrance have their heads turned in towards it, those within have them turned in the opposite direction.

We may affirm, that they arrange themselves regularly to ventilate more at ease. Thus they form files, terminating at the entrance of the hive, and sometimes disposed like so many diverging rays. But this order is not uniform. Probably it is owing to the necessity for the ventilating bees giving way to those going and coming, whose rapid course compels them to range themselves in a file, to avoid being hurt or thrown over every instant.

Sometimes above twenty bees ventilate at the bottom of a hive, at other times their number is more circumscribed, and their employment of various duration. We have seen them engaged in it during twenty-five minutes, only taking breath, as it were, by the shortest interruption of the vibration. On ceasing, they are succeeded by others, so that there never is any intermission of the buzzing in a populous hive.

If under the necessity of ventilating during winter, near the centre of the mass then united towards the top of their dwelling, doubtless the bees perform this important function among the vacuities of the irregular combs, where there is room for their wings to expand, as at least half an inch is requisite for them to play freely.

Is ventilation as essential to bees in their natural state, when inhabiting hollow trees or the more spacious cavities of rocks, as in their domesticated condition? In imitation of such situations, we lodged a swarm in a glass hive, five feet high, which on all sides exposed their pyramidal cluster suspended from the combs above. A common entrance was made at the bottom. We observed that very few were occupied here in fanning themselves—it was always on the side of the same quarter where the greatest numbers of the swarm had accumulated, they kept at a little distance from each other, and in the way of those returning from the fields.

The ventilation of the bees, or the buzzing which denotes it, seems to me more active during winter than at any other time. It was easy to prove that this operation established a current of air, for anemometers of light paper or cotton, hung by a thread, were impelled towards the entrance, and repelled from it with equal rapidity. The action on them never was entirely interrupted, and its force appeared proportional to the number of bees fanning themselves.

If some cultivators shut up the entrance of their hives in winter without prejudice to the bees, it must be considered that the air will penetrate, nevertheless, through the straw composing them. I confided an experiment on this head to Burnens, then at a distance from me, who communicated the result in a letter, dated *Oulens, 3d Feb.* 1797. Having closed down a very populous straw hive fast on its board, he found that a piece of the finest paper, suspended by a hair before the entrance, oscillated above an inch off the perpendicular line. He poured liquid honey through an opening in the top, when a buzzing soon began, and the tumult increasing within, several bees departed. The oscillations now became stronger and more frequent. His experiments were made at three o'clock, the sun shining bright, and the thermometer in the shade standing at 44 degrees.

ARTICLE IV. *Proofs from an artificial ventilator.*—Although the purpose of ventilation is evident from the preceding experiments, M. Senebier advised me to obtain a similar effect from an artificial ventilator. Mr Schwepp, who is both an expert mechanic, and an ingenious naturalist, aided me in constructing a small windmill, with eighteen tin vanes, put into a box, on the top of which a large cylindrical vessel was adapted.

Experiment 1, 2. An aperture representing the entrance of a hive was left free, but a taper in the vessel was extinguished in eight minutes, though its capacity was equivalent to 3228 inches. A single aperture, therefore, does not promote the renewal of air, unless it be agitated from some external cause.

3. Fresh air was restored, and a taper introduced,

when the ventilator being put in action, several anemometers, suspended before the entrance, proved the establishment of two currents. During the whole course of the experiment, which may be prolonged indefinitely, the brilliancy of the light remained undiminished. A thermometer, at the bottom of the apparatus, stood at 122 degrees; the temperature in the receiver above evidently was higher.

4. Wishing to try whether my ventilator would correct the vitiation of the air by two tapers, I found that they burnt fifteen minutes, and that both went out at once. In another experiment, when the ventilator was still, they were extinguished in three minutes.

5. Contrary to our expectations, a more powerful current of air was not obtained by multiplying the apertures.

These experiments, showing that the air can be renewed in a vessel with apertures on only one side, when it is displaced by a mechanical cause, seem to confirm our conjectures regarding the ventilation which bees can produce in their hive.

ARTICLE V. *Immediate source of ventilation.*—In striking the air with their wings, bees know little of the real object attained. Perhaps some inclination or some very simple necessity induces them to put organs in motion, apparently bestowed on them only for flight. Doubtless it is to relieve an immediate sensation, for we cannot grant them the same knowledge that would induce us to act after a relative manner. Nevertheless, it is curious to learn those propensities, however gross, by means of which Nature enables them to accomplish an object.

The bees fanning themselves for coolness may be one immediate cause of ventilation. We remarked that the rays of the sun, having darted on combs covered with bees, from opening the shutter of a glass hive, those in the shade remained in tranquillity, while those sensible of the heat began buzzing. Likewise, the bees of clusters formed before the hive, in summer, fan themselves vigorously if incommoded by the heat of the sun; but the ventilation ceases on shading part of the clusters, though it continues in the rest.

The same may be observed in humble bees, an analogous genus. Those kept with their nest in a window, peaceable in general, began to be very noisy as the rays of the sun fell on the box containing them —all vibrated their wings and buzzed loudly. Sometimes, also, we hear this sound in the nests of wasps and hornets. Thus it appears certain that it is heat which induces bees and other insects to fan themselves.

But it is singular that they ventilate, likewise, during the cold of winter, when heat may be only a secondary cause.

We found that the influence of obnoxious odours led them to fan themselves. Having separated some bees from their hive by the attraction of honey, we brought cotton, dipped in spirit of wine, near the head. Its effects could not be mistaken—they dispersed, agitating their wings, and then drew together again to resume their repast. When completely engaged the experiment was repeated, and they dispersed anew, but without retracting the proboscis entirely—they were satisfied with vibrating the wings, and continued feeding.

When too much affected by disagreeable sensations, they precipitately withdrew, and took flight. Frequently a bee turned away from the honey, and began to fan itself, until the sensation or its cause were by this means abated, and then returned to feed.

Such experiments never are more successful than at the entrance of the hive ; because the bees retained by the united attraction of honey and their home, are less disposed to retreat from external impressions. Humble bees adopt the same method of dispelling pernicious odours. But what is very remarkable, and may partly show the importance of vibrating the wings, neither their males nor those of domestic bees, though very sensible of similar emanations, know to protect themselves, like the workers, against them.

Ventilation, therefore, is one of the industrial operations peculiar to the workers.

The Author of Nature, in assigning a dwelling to these insects where the air can hardly penetrate, bestows the means of averting the fatal effects which might result from the vitiation of their atmosphere. Perhaps the bee is the only creature entrusted with so important a function, and which indicates, thus to speak, such delicacy in its organization.

An indirect consequence of ventilation is the elevated temperature of the hive, preserved by the bees without any exertion, and resulting, like the heat of all animals, from their respiration. This, which one author gratuitously ascribes to the fermentation of the honey, certainly is derived from the accumulation of a great many bees in the same place. It is so essential to them and their young, that it ought to be independent of the temperature of the atmosphere.

The existence of bees thus depends in more than one point on the constancy of ventilation. Yet while called on to execute so many different labours, each member of the colony cannot be occupied, on its own account, with the care of preserving the air at the necessary degree of purity. This function being exercised by a few individuals alternately, does not abridge the other branches of industry indispensable.

Thus the condition of bees admitting of their fulfilling by turns the different functions imposed on the whole society, corresponds to the beneficent views of the Creator, and supplies the place of the arrangements which we ourselves would make for our own advantage.

CHAPTER V.

ON A NEW ENEMY OF BEES.

Among the labours of insects, those which concern the defence of their habitations are not the least deserving the attention of man, who is so often called on to fortify himself against the enterprises of his enemies. It is here that Nature unfolds the most unexpected resources, where she admits of the greatest latitude; for the chances of war are the object of one of those general laws concurring to the preservation of universal order. Without the alternatives of success and reverse, how could an equilibrium be maintained? One species endowed with superior strength would annihilate another. Nevertheless the most timid have subsisted since the origin of things—their tactics, their industry, their fecundity, or other circumstances peculiar to their kind, have enabled them to escape that extinction which seems to menace them.

Among bees, as with the greater part of their class, the ordinary means of resistance are those poisoned weapons with which they wound their enemies. The fate of war would be more in their favour, were not several of their antagonists armed still better than themselves—if others had not the art of shunning their vigilance by sheltering themselves under a covering—and were there not also some which can profit, by the weakness of an ill-peopled hive, to gain a surreptitious introduction into it.

Wasps, hornets, the larvæ of moths, and mice, have been known from all antiquity by their ravages among hives; and having nothing to add to what is generally said respecting them, I shall confine myself to pointing out a new enemy of bees.

Towards the close of summer, after having stored up part of their collections, we sometimes hear a surprising noise in their habitations. A multitude of workers depart through the night, and lose themselves in the air: the tumult frequently continues during several hours, and on examining into the consequences of so great an agitation in the morning, we find numbers dead before the hive. For the most part, the honey is observed to be exhausted, and occasionally the hive is quite deserted.

In the year 1804, many of my neighbouring cultivators came to consult me on an occurrence of this nature. But I could give them no explanations; notwithstanding my long practice in studying bees, I never had seen any thing similar.

On visiting the scene, I found the phenomenon still subsisting, and that it had been very accurately described; but it was assigned to the introduction of bats into the hives, which I had difficulty in crediting. These creatures are satisfied with seizing nocturnal insects in their flight, which are never wanting in summer. They do not feed on honey; why, therefore, should they attack bees in their habitations, for the purpose of pillaging their stores?

However, it might be some other animal. Therefore, having put my people in ambush, they soon brought me intelligence, not of bats indeed, but of the sphinx atropos, or death's head, a large moth flying in numbers about the hives; and one was caught at the moment of attempting to penetrate a hive among the least populous, where it evidently designed living at the expense of the bees.

From all quarters I had information of similar ravages committed by bats, as supposed. Cultivators, who expected a plentiful harvest, found their hives as light as in the first days of spring—though recently well provisioned, they were now reduced to the weight of wax.

At length, the gigantic moth which had occasioned the desertion of the bees was surprised in several hives.

Such reiterated proofs were requisite to persuade me that a butterfly, an insect wanting a sting, unprovided with any shield or other means of defence, could contend victoriously against thousands of bees; but this year they were so common, that it was easy to be convinced of the fact.

As the enterprises of the sphinx constantly became more fatal to the bees, we resolved to prevent its access by contracting the entrance of the hive by a kind of grating, large enough to admit none but its proper tenants. This plan had complete success—quiet was restored, and the devastation ceased.

The same precautions had not been universally taken; but we observed that the bees, left to themselves, had provided for their own security. Without any foreign aid, they had barricaded themselves, by means of a thick wall of propolis and wax, rising behind the entrance of the hive, sometimes in the entrance itself, and completely obstructing it, but penetrated by passages for one or two workers at a time.

The operations of man and of the insect had completely coincided.

The works which the bees had established were of very various formation. Here was a single wall, whose opening arcades were disposed in its higher part—there several bulwarks behind each other recalled the bastions of our citadels. Gateways, masked by walls in front, opened on the face of those of the second row, while they did not correspond with the apertures of the first. Sometimes a series of intersecting arcades permitted free egress to the bees, but refused admittance to their enemies. These fortifications were massy—their substance firm and compact.

As such casemated gates are not constructed by bees without urgent necessity, we cannot ascribe their proceedings to any of those demonstrations of prudence prepared to obviate inconveniencies, which the insect neither can know nor anticipate. It is only when danger is present, when it is pressing and immediate, that, compelled to seek protection, it employs this last resource. Thus it is curious to observe an insect, so well armed and supported by the advantage of numbers, securing itself by an admirable combination against the inefficiency of its weapons and its courage. The art of warfare among bees, therefore, is not restricted to attacking their enemies: they know also to construct ramparts, as shelter from their enterprises—from the part of simple soldiers they pass to that of engineers.

But it is not against the sphinx alone that they must be guarded. Weak hives are sometimes attacked by stranger bees, attracted by the odour of the honey and the hopes of easy pillage. Those beset being unable to defend themselves from this invasion, are known to have recourse to a measure resembling that employed against the hostile moth. Then, also, they raise walls, leaving only narrow openings for the passage of a single bee at a time, and which therefore can be easily protected.

But a period arrives when these galleries are no longer suitable to the bees. At the time that their harvest is abundant, their hive excessively populous, and the formation of new colonies approaches, they demolish the gateways which had been erected in the hour of danger, and which now restrain their impetuosity. Such safeguards have become inconvenient, and they are removed, until new alarms demand their reconstruction.

The entrances formed in 1804 were destroyed in spring 1805. The sphinx did not appear that year, nor was it seen in the year following; but it returned in great numbers in autumn 1807. By speedily barricading themselves, the bees prevented their threatened ravages; but, before the departure of swarms in May 1808, they demolished the fortifications, whose narrow passages prohibited free egress to the multitude.

When the entrance to the hive is itself restricted, or care being taken to contract it soon enough to prevent the devastation of their enemies, bees dispense with walling themselves in. We cannot explain their conduct otherwise than by admitting the evolution of their instinct according as excited by circumstances.

But how can the sphinx alarm colonies so warlike?

How can a moth, the dread of superstitious people, also exercise a secret influence over insects, and have the faculty of paralyzing their courage? Does it emit any emanation pernicious to bees?

Other species of sphinxes subsist on the nectar of flowers alone. They have a long, slender, flexible, spiral trunk, and seek their food at sunset. But the atropos is later on the wing, nor does it hover about the hives until night is farther advanced. It is provided with a thick, short proboscis; is endowed with great strength; and when seized, some unknown organ emits an acute stridulous sound. May not this, which inspires the vulgar with sinister ideas, be also the dread of bees? May not its resemblance to that emitted by the queen in her captivity, which has the faculty of suspending the vigilance of the workers, explain the disorder observed in their hive on the approach of the sphinx? But this is only a conjecture, founded on the analogy of sounds, to which I attach no importance. Meantime, were any piercing notes observed to proceed from the sphinx during its assaults, and that the bees then yielded without resistance, my conjecture would acquire some weight.*

The introduction of a butterfly so large and recognisable as the sphinx atropos into a well-peopled hive, and the extraordinary consequences thence resulting, are phenomena of the more difficult explanation, from

* Reaumur ascribes the sound to the friction of the trunk against its sheaths, but we have ascertained that this organ has no share in it. Though many naturalists have investigated its source, nothing satisfactory is known on the subject. It is undoubted that the sphinx emits the sound at pleasure, and particularly when affected by the apprehension of danger.

nothing in the organization of the insect indicating that it is screened from the sting of bees.

We have been anxious to witness this singular contest in glass hives, but no opportunity has hitherto offered. However, to solve some of my doubts, I have made a few experiments on the mode in which the sphinx is received by humble bees.

Having procured some of the largest size, I introduced them at nightfall into a glass box, where a colony of small brown humble bees *(muscorum)* had been established. The first carried thither did not appear to be affected by the smell of the honey; it remained quiet in a corner. Beginning to approach the nest and its inhabitants, it soon became the object, not of the dread, but of the wrath of the workers. Repeatedly assailing it with fury, they gave it frequent stings. It sought to escape; it run quickly; and at last raising the glass cover of the apparatus by a violent exertion, succeeded. It seemed to suffer very little from its wounds,—remained tranquil all the night, and several days after was wonderfully well.

Another sphinx, very vigorous and lively, which often emitted the sound peculiar to its species, was confined along with the same colony. Its activity only served to render it the sooner the victim of their rage. Immediately on approaching the nest, which nevertheless it seemed to have no desire to enter, all the workers darted on it at once with their stings, and harassed the creature so incessantly as to compel it to retreat. The moth defended itself only by the violent agitation of its wings, but it could not evade the attacks of the humble bees under the belly, where it seemed most sensible of their weapons. At length, after an hour of suffering, it perished under many wounds.

I was unwilling to carry this cruel experiment farther. Captivity, or some other circumstance, evidently reduced the insect to too great an inferiority to the humble bees. Yet, after the experiment, it became still more difficult for me to understand how it could introduce itself into the hives of common bees, whose stings are so much more dangerous, as well as their numbers incomparably greater. Had the light of a torch been an obstacle to the sphinx exercising its means of attack? Possibly the success of its enterprises on hives results from the faculty of seeing during the night, like other moths of the same genus.

Offering honey to these insects was equally fruitless. They remained during a week beside a comb without touching it. We unfolded the proboscis, and dipped it among honey, in vain. This experiment, though succeeding perfectly with day butterflies, proved abortive with the sphinx atropos.

Had I not obtained proofs of its avidity while in the natural state, I might have entertained doubts of the predilection for honey. Besides, the facts above related are supported by my having recently dissected a large sphinx taken in the open air, and found the abdomen quite full of pure honey, of the same taste and consistence as that of bees. The quantity would have filled a table-spoon; and what appeared very singular, was its not being contained in any particular intestine, but occupying the cavity usually reserved for air in the body of these insects. All the vertical delicate membranes dividing the abdomen into so many compartments had disappeared. I cannot affirm positively whether they had been ruptured by the quantity of honey gorging the sphinx, or by ourselves; but one thing is undoubted, that in others of the sphinx atropos, opened by us, we have always seen the receptacles entire though empty.

From facts illustrating the history of the sphinx, let us return to the means of preserving bees from so dangerous an enemy.

This may be accomplished by the aid of a small slider at the entrance of the hive, penetrated by three kinds of apertures, proportioned to the necessities of the bees, and adapted to use in a manner corresponding with their own plans of security.

As they destroy their fortifications in spring, previous to the flight of the swarms, then we ought to leave the entrance free. They have few enemies to dread, and their community, well peopled, can defend itself. After departure of the swarms the entrance is to be contracted, because the hive being weakened, stranger bees or moths may introduce themselves; and our proceedings are pointed out by the works of the bees when menaced with pillage.* In the middle

* The author elsewhere observes, that, on the 9th of July 1804, some dead and wounded bees were found on the board of the hive, which were recognised to have come for the purpose of pillage. On the 11th, however, the entrance was built up by the bees with wax, two openings being left at the part farthest above the board, which would allow only a single bee to pass at a time. Thus, two of their own number always were enough to guard it. These apertures were enlarged against the 22d, so as to admit the passage of two or three bees at once; and another opening was made in the course of September. The author advises cultivators to visit their hives frequently, and, on finding them pillaged, to feed the bees with

of July these entrances are so much enlarged by them, that two or three of their number can pass at once, and they allow the free egress of the males, which are of larger size than the workers. At this epoch, therefore, it is necessary to advance the slider so far as to bring the apertures, whose convexity should be downwards, opposite to the entrance of the hive. Finally, the collection being in its full force in the months of August and September, it is essential that the bees ought not to be too much confined. Those whose example we follow opened a third passage in the waxen wall, resembling a very low vault, which we should imitate in the third row of apertures. By this means the access of the sphinx is precluded, while the bees can find an easy exit. If the slider be made of tin instead of wood, it will exclude mice, another most dangerous enemy to bees.*

In subduing animals, man in some measure impairs the equilibrium establisbed by Nature among rival species, and more or less diminishes their energy and vigilance. The features of their instinct can be then discovered only by judging of their peculiarities ; and, in his turn, he should partly compensate the advantages of which they have been deprived. He must do more, if wishing to augment their products, for he has to contend with Nature, which assigns limits to

honey and syrup, without which he never was able to preserve them. Daily supplies melted should be introduced in nutshells from behind.—*T.*

* In September 1802, M. Lombard found the entrance of all his hives almost totally closed up by the bees ; but at that time he did not know to what he should ascribe it.—*Manuel des Proprietaires d'Abeilles*, p. 32.—*T.*

the multiplication of individuals. But this demands a very profound knowledge of the wants of the creatures subjected to his dominion, and of the resources which Providence has put within their reach; for it is from themselves that the art of managing them must be learned.

CHAPTER VI.

NEW OPINIONS ON WAX.

SINCE the time of Reamur and De Geer, whose works have inspired a general taste for entomology, great advances have been made in this science ; all its branches are extended, and the history of bees, in particular, has been enriched.

Schirach and Riem have opened a new path, and, perhaps, we have ourselves contributed to clear it of the prejudices which clogged its progress, by establishing the facts announced in a more rigorous manner.

Some observations, also, have been published in other countries, but so inaccurately that they would sink into oblivion did we not endeavour to support them by facts.

Naturalists have principally directed their attention to wax, and chemists likewise have attempted an analysis of it ; but the result of their labours presents so little coincidence, as to prove the insufficient discussion of the subject, and that it requires new examination.

When M. Bonnet wrote, it was the general opinion that the pollen, farina or dust of the stamina of flowers was converted into wax, and it is interesting to peruse his details of its collection, the manner in which the bees load themselves, and how they store it up for preservation. All these facts, and the utility of the farina, had been scrupulously observed by Reaumur, Maraldi, and other learned men; but is this substance truly the elementary principle of wax?

Reaumur entertained some hesitation regarding it, from the great difference between the pollen and the wax; however, he inclined to believe that the former, by receiving some peculiar elaboration from the bees, was converted into real wax in the stomach, and disgorged under the appearance of a kind of paste.

A Lusatian cultivator, whose name has not reached us, observes, that although it had been previously supposed that wax is discharged from the mouth, it actually comes from the rings of the abdomen; and that this is evident by withdrawing a bee from the cell where it works in wax, when the wax in the form of scales appears by the extension of the body.*

In the year 1793, we were greatly astonished at finding scales of a substance analogous to it under the rings. They exhibited its real characteristics on being applied to the flame of a taper, and we showed them to some of our friends.

John Hunter, an Englishman of high reputation, engaged in observations on bees at the same period, discovered the actual receptacles of wax under the belly of these insects. He found a flexible matter in them, which he recognised as such; he ascertained the difference between the farina or pollen of flowers

* Schirach's History of the Queen Bee.

and the composition of the combs, and assigned a new property to the pellets carried home on the legs of bees. But he could only offer conjectures on the use of the pollen; nor had he witnessed the application of the scales which, according to his supposition, transuded from the body. Our inquiries were carried farther.

Workers alone have the property of secreting wax; scales of it, ranged in pairs, are contained in minute receptacles under the lower segments of the abdomen, and situated to right and left of the angular projection. The conformation of the same part of queens and males is very different, below the rings of which no scales subsist.

Nothing but what is common to the abdomen of wasps and many other hymenoptera appears externally in that of the bee, being half segments partially covering each other. But they are not flat below as in most analogous insects, for the abdomen of the bee is traversed by an angular prominence. Plate II., fig. 10, a, b. By gently drawing out the abdomen, the concealed parts are disclosed, fig. 9, 10, 11, 12.

What should be considered the base of each ring, because it adheres to the body of the insect, occupies at least two-thirds of the segment, and is of a yellowish white, soft, transparent, membranaceous substance. Plate III., fig. 13, c, d, e, g. It is divided in two by a small horny prominence, a, b, corresponding exactly with the horny projection of the abdomen, and forms two areas bounded by a solid edge on the surface of the membrane, n, c, b, a, d,—m, g, a, b, e. The scales of wax are deposited in these two areas, and assume the same conformation, being of an irregular pentagonal figure. Only eight scales belong to each individual bee, for the first and last ring, constituted differently from the others, afford none. Their size decreases with the diameter of the rings whereon they are moulded; the largest are under the third, and the smallest under the fifth. All are not alike in every bee, for a difference is perceptible in consistence, shape, and thickness; some are so thin and transparent as to require a magnifier to be recognised; or we have been able to discover nothing but spiculæ similar to those of water freezing.

Neither the spiculæ nor scales rest immediately on the membrane; a slight liquid medium is interposed, serving to lubricate the junctures of the rings, or to render the extraction of the scales easier, as otherwise they might adhere too firmly to the sides of the receptacles.

Finally, we have seen the scales so large as to project beyond the rings, being visible without stretching the segments, and of a whitish yellow, from greater thickness lessening their transparency.

These shades of difference in the scales of various bees, their enlarged dimensions, the fluid interposed beneath them, the correspondence between the scale and the size and form of the receptacles, seem to infer the transudation of its substance through the membranes whereon it is moulded.

We were confirmed in this opinion by the escape of a transparent fluid, on piercing the membrane, whose internal surface seemed to be applied to the soft parts of the belly. It coagulated in cooling, when it resembled wax, and again liquified on exposure to heat.

The scales themselves also melted and coagulated like wax.

In prosecuting our experiments farther on the analogy of the two substances, we found, 1. That scales, thrown into spirit of turpentine, dissolved and disappeared before reaching the bottom of the vessel, without rendering the fluid turbid. But an equal quantity of the spirit could neither dissolve some of the whitest fragments of worked wax taken from new combs so quickly nor so entirely. Many particles remained suspended among it.

2. Taking two vessels of sulphuric ether, we appropriated one for scales from the rings of bees, the other for wax from their combs, equivalent in weight to the scales. Scarcely had fragments of the wax touched the ether when they divided, and were precipitated in powder to the bottom of the vessel; but the scales were preserved entire, and only lost their transparency, becoming of a dull white. No change ensued in either vessel during several days. On evaporating the ether from each, a thin stratum of wax was found on the glass. Frequent repetition of this experiment presented the same result; fragments of the combs always were reduced to powder; the scales, on the contrary, were not broke down; and after the lapse of several months, only a very small portion of them had been dissolved by the ether.

We thence concluded that the wax of the rings was less compound than that made into cells, since the latter dissolved in ether, while the former remained entire, and as the one dissolved but partially in the spirit of turpentine, whereas the other was held in complete solution.

If this substance lying under the rings be truly the elements of wax, it undergoes some preparation in leaving its receptacles, and the bees are capable of impregnating it with matter, imparting the whiteness and ductility of real wax. Hitherto we are acquainted only with its fusibility, but such being the chief property of the wax of the combs, we cannot doubt that the scales enter their composition.

We have been disappointed of reaching the sources of the waxy matter by dissections, though executed by the skilful hand of Miss Jurine. No direct communication between the receptacles and the abdomen could be discovered; nor were any vessels found, except some tracheæ, certainly for the introduction of air. But the membrane of the receptacles is covered with a reticulation of hexagonal meshes, to which we should ascribe a certain function connected with the secretion of wax. Plate III., fig. 15, 16. This is wanting in males, but it exists in queens, under a modified texture, and occupying two-thirds of each segment.

A reticulation quite similar to that of the working bee, and occupying the whole anterior part of the segments, is found in humble bees producing wax; but the receptacles are imperceptible; their abdomen being formed like that of the hymenoptera of the same genus.

The stomach and other internal parts are separated from the reticulation by a greyish membrane entirely covering the abdominal cavity of the common bees. When the stomach is full of the juices elaborated by it, they may transude its very thin integuments, and traversing the greyish membrane, which is not thick, they may come in contact with the reticulation. It is not impossible that the reticulation operating a re-absorption and a kind of digestion of these juices, the secretion of wax follows.

Though direct evidence be yet wanting, we may admit that this substance is produced by a particular organ, after the manner of other secretions.

CHAPTER VII.

ON THE ORIGIN OF WAX.

THE existence of the organs above described, and the scales seen under different gradations, induce us to believe them appropriated for the secretion of wax. But, in common with other animal and vegetable secretions, the means by which this is accomplished appears to be carefully veiled by Nature.

Our researches by simple observation thus being obstructed, we felt it essential to adopt other methods for ascertaining whether wax actually is a secretion, or the collection of a particular substance.

Providing it were the former, we had first to verify the opinion of Reaumur, who conjectured that it came from an elaboration of pollen in the stomach, though we did not coincide with him that bees then disgorged it by the mouth. Neither were we disposed to adopt his sentiments regarding its origin; for, like Hunter, it had struck us that swarms newly settled in empty hives do not bring home pollen, notwithstanding they construct combs, while the bees of old hives, having no cells to build, gather it abundantly.

We had, therefore, to learn whether bees, deprived of pollen for a series of time, would make wax, and all that this required was confinement.

On the 24th of May we lodged a swarm which had just left the parent stock in a straw hive, with as much honey and water as necessary for the consumption of the bees, and closed the entrances so as to prevent all possibility of escape, leaving access for renewal of the air.

At first the bees were greatly agitated; but we succeeded in calming them by carrying the hive to a cool dark place, where their captivity lasted five days. They were then allowed to take flight in an apartment, the windows of which were carefully shut, and where the hive could be examined conveniently. The bees had consumed their whole provision of honey; but their dwelling, which did not contain an atom of wax when we established them in it, had now acquired five combs of the most beautiful wax suspended from its arch, of a pure white, and very brittle.

We did not expect so speedy a solution of the problem; but before concluding that the bees had derived the faculty of producing wax from the honey on which they fed, a second experiment, susceptible of no other explanation, was necessary.

The workers, though in captivity, had been able to collect farina; while they were at liberty they might have obtained provisions on the eve or on the day itself of their imprisonment, and enough might have been in the stomach or on the limbs to enable them to extract the wax from it that we had found in the hive. But if it actually came from the farina previously collected, this source was not inexhaustible; and the bees being unable to obtain more, would cease to construct combs, and would fall into absolute inaction.

Before proceeding to the second experiment, which was to consist in prolonging their captivity, we took care to remove all the combs they had formed in that preceding. Burnens made them return to the hive, and confined them again with a new portion of honey. The experiment was not tedious. From the evening of the subsequent day we observed them working in wax anew; and on examining the hive on the third day, we actually found five combs as regular as those they had made during their first imprisonment.

The combs were removed five times successively, but always under precaution of the escape of the bees from the apartment being prevented; and during this long interval, the same insects were preserved and fed with honey exclusively. Undoubtedly, the experiment, had we deemed it necessary, might have been prolonged with equal success. On each occasion that we supplied them with honey they produced new combs, which puts it beyond dispute that this substance effected the secretion of wax in their bodies, without the aid of pollen. As the reverse of the preceding experiment would prove whether the pollen itself had the same property, instead of supplying our bees with honey, we fed them on nothing except fruit and farina. They were kept eight days in captivity, under a glass bell with a comb, having only farina in the cells; yet they neither made wax, nor were scales seen under the rings. Could any doubt exist as to the real origin of wax?—We entertained none.

It was not improbable that wax might be contained in honey, and reserved for use by the bees as requiring it, for some particles always rise to the surface of honey diluted with water. But the microscope indicated them to be fragments of cells previously made.

However, to obviate any objection, and to ascertain whether the saccharine principle was the real source of wax, we supplied a swarm confined in a glass hive with a pound of refined sugar reduced to syrup. In order to render the experiment more instructive, by comparison, two swarms were introduced into other two hives, one of which we fed with very dark brown sugar, and the other with honey.

The result proved as satisfactory as could be expected—wax was obtained in all the three. Those bees that had been fed with the different kinds of sugar produced it sooner, and in greater abundance, than those that had subsisted on honey.

A pound of refined sugar, reduced to syrup, and clarified with eggs, produced 10 drams 52 grains of wax, darker than that extracted by the bees from honey. An equal weight of dark brown sugar produced 22 drams of very white wax—the like came from sugar of the maple.*

We repeated this experiment seven times successively with the same bees—wax was always obtained, and nearly in the same proportions as above. Thus it is demonstrated that sugar, and the saccharine part of honey, enable bees to produce wax, a property denied to farina.

Although there was no uncertainty regarding these

* That is, two ounces and three quarters was the greatest quantity of wax obtained from a pound of sugar, or nearly one-sixth of the weight.—*T.*

facts, which soon received a more favourable confirmation, it was essential to learn whether bees, in the natural state, pursued the same course as those held captive.

A long series of observations, of which only a sketch shall be given, have established, that when the state of the country affords a copious collection of honey, it is anxiously stored up by the workers of old hives, while new swarms convert it into wax.

At a time when I had not a great number of hives myself, those of the neighbouring peasantry served for comparison, though made of straw, and wanting the facilities of my own. Certain remarks, which we had made on the combs and on the bees themselves when working in wax, enabled us to avail ourselves of them.

Wax is originally white, but the cells soon become yellow; they grow brown in time, and the combs of very old hives have a blackish hue. Thence we can immediately discover whether bees are working in wax, or whether their labours are suspended, merely by raising the hives to see the lower edge of the combs.

The following observations, founded on the fact of there being two kinds of workers in a hive, which was unknown to my precursors, also may afford some indications of the presence of honey in the flowers. One of these is, in general, destined for the elaboration of wax, and its size is considerably enlarged when full of honey; the other immediately imparts what it has collected to its companions, its abdomen undergoes no sensible change, or it retains only the honey necessary for its own subsistence. The particular function of the bees of this kind is to take care of the young, for they are not charged with provisioning the hive. In opposition to the wax workers, we shall call them small bees or nurses.

Although the external difference be inconsiderable, this is not an imaginary distinction. Anatomical observations prove that the capacity of the stomach is not the same—experiments have ascertained that one of the species cannot fulfil all the functions shared among the workers of a hive. We painted those of each class with different colours, in order to study their proceedings; and these were not interchanged. In another experiment, after supplying a hive deprived of a queen with brood and pollen, we saw the small bees quickly occupied in nutrition of the larvæ, while those of the wax working class neglected them.

When hives are full of combs, the wax workers disgorge their honey into the ordinary magazines, making no wax; but if they want a reservoir for its reception, and if their queen does not find cells ready made, wherein to lay her eggs, they retain the honey in the stomach, and in twenty-four hours wax exudes through the rings. Then the labour of constructing combs begins.

Perhaps it will be supposed that, when the country does not afford honey, the wax workers may consume the provision stored up in the hive. But they are not permitted to touch it. A portion of honey is carefully preserved, and the cells containing it are protected by a waxen covering, which never is removed except in case of extreme necessity, and when honey is not to be otherwise procured. The cells are at no time opened during summer; other reservoirs always exposed contribute to the daily use of the community; each bee, however, supplying itself from them with nothing but what is required by present exigence.

Wax workers appear with large bellies at the entrance of their hive only when the country affords a copious collection of honey.

From what we have said, it may be concluded that production of the waxy matter depends on a concurrence of circumstances not invariably subsisting.

Small bees also produce wax, but in a very inferior quantity to what is elaborated by the real wax workers.

Another characteristic, whereby an attentive observer can determine the moment of bees collecting sufficient honey to produce wax, is the strong odour of both these substances from the hive, which is not equally intense at any other time.

From such data, it was easy for us to discover whether the bees worked in wax in our own hives, and in those of the other cultivators of the district.

The inclemency of the season in 1793 had retarded the departure of swarms until after the 24th of May. Most of the hives swarmed in the middle of June, the country being covered with flowers. Much honey was collected by the bees, and the new swarms worked actively in wax.

Burnens inspected sixty-five hives on the 18th, and saw wax workers before the entrance of all. Those returning to the old hives speedily stored up their collections, without constructing combs; but those of the swarms converted their honey to wax, and hastened to prepare receptacles for the eggs of their queen.

The 19th was showery, and, although the bees went out, they brought home nothing but pollen. The weather continued chill and rainy until the 27th, when we became desirous of learning what had resulted from it. Burnens, raising all the hives on the 28th, found that the labour of the bees had been interrupted—that the combs measured on the 9th had received no accession, they were of citron yellow, nor were white cells in any of the hives.

Wax workers re-appeared on the first of July, the weather being milder, and the chesnut and elm having come in flower. They collected much honey—the swarms enlarged their combs—great activity prevailed—and these labours continued until the middle of the month. But on the 16th, the thermometer rising above 77 degrees, drought commenced—the flowers of the meadows, together with those of the chesnut and elm, had faded entirely. No longer containing honey, their pollen alone attracted the bees, and was copiously collected; but they produced no wax. The combs were not enlarged—those of the swarms remained stationary.

Previous to the 10th of August it had not rained for six weeks, neither did nocturnal dews temper the extraordinary heat. The bees found pollen alone, but no honey, in the flowers of black grain, which now had been spread during several days. After some hours rain on the 10th, they exhaled the odour of honey, and we actually saw it glittering on the faded flourish. The bees found enough for their subsistence, but too little to induce them to work in wax.

The drought returning on the 14th, continued until the end of the month, when we examined the sixty-five hives for the last time. It appeared that the bees had prepared no wax from the middle of July; that they had stored up much pollen; that the provision of honey had diminished considerably in the old hives, and that there was scarcely any in the new.

The year, therefore, was very unfavourable to the labour of bees, which I ascribe partly to the atmosphere not being charged with electricity, which has great influence on the secretion of honey in the nectarium of flowers. I have remarked that the collection by these creatures is never more abundant, nor their operations in wax more active, than when the wind is from the south, the air moist and warm, and a storm approaching. Heat too long protracted, however, and its concomitant drought, chill rains, and a north wind, entirely suspend the elaboration of honey in vegetables, and consequently the work of bees.

When bees were confined, for the purpose of discovering whether honey was sufficient for the production of wax, they supported their captivity patiently, and showed uncommon perseverance in rebuilding their combs according as we removed them. Had part of the combs been left, the queen would have laid in the cells—we would have seen in what manner the workers conducted themselves towards the young, and what the latter suffered from total privation of the farina. But occupied at the time solely regarding the origin of wax, we preferred a separate inquiry into the subject of rearing their young.

Our experiments here required the presence of larvæ honey and water had to be provided; the bees were to be supplied with combs containing brood, and at the same time it was necessary to confine them, that they might not seek pollen abroad. Having a swarm by chance, which had become useless from sterility of the queen, we devoted it for our investigation in one of my leaf hives, which was glazed on both sides.

We removed the queen, and substituted combs full of brood for those of the first and last division, that is, containing eggs and young larvæ, but no cell with farina; the smallest particles of the substance, which John Hunter conjectured to be the basis of the nutriment of the young, were even taken away.

Nothing remarkable occurred during the first and second day: the bees brooded over the young, and seemed to take an interest in them; but at sunset, on the third, a loud noise was heard in the hive. Impatient to discover the reason, we opened a shutter, and saw all in confusion; the brood was abandoned—the workers ran in disorder over the combs—thousands rushed towards the lower part of the hive, and those about the entrance gnawed at its grating. Their design was not equivocal—they wished to quit their prison. Some imperious necessity evidently obliged them to seek elsewhere what they could not find in the hive; and apprehensive that they might perish if I restrained them longer from yielding to their instinct, I set them at liberty.

The whole swarm escaped, but the hour being unfavourable for their collections, they flew around the hive, and did not depart far from it. Increasing darkness and the coolness of the air compelled them very soon to return. Probably these circumstances calmed their agitation, for we observed them peaceably remounting their combs; order seemed re-established, and we profited of this moment to close the hive.

Next day, the 19th of July, we saw the rudiments of two royal cells, which the bees had formed on one of the brood combs.

This evening, at the same hour as on the preceding, we again heard a loud buzzing in the close hive—agitation and disorder rose to the highest degree, and we were again obliged to let the swarm escape. The bees did not remain long absent from their habitation; they quieted and returned as before.

We remarked on the 20th, that the royal cells had not been continued, as would have been the case in the ordinary state of things. A great tumult took place in the evening, the bees appeared to be in a delirium; we set them at liberty, and order was restored on their return.

Their captivity having endured five days, we thought it needless to protract it farther; besides, we were desirous of knowing whether the brood was in a suitable condition, if it had made the usual progress; and we wished also to try to discover what might be the cause of the periodical agitation of the bees.

Burnens, therefore, having exposed the two brood combs, the royal cells were immediately recognised; but it was obvious that they had not been enlarged. Why should they? Neither eggs, worms, nor that kind of paste peculiar to the individuals of their species were there! The other cells were vacant likewise, no brood, not an atom of paste was in them. Thus the worms had died of hunger.

Had we precluded the bees from all means of sustenance, by removing the farina?

To decide this point, it was necessary to confide other brood to the care of the same insects, now giving them abundance of pollen. They had not been enabled to make any collections while we examined their combs. On this occasion they escaped in an apartment where the windows were shut; and after substituting young worms for those they had allowed to perish, we returned them to their prison.

Next day we remarked that they had resumed courage—they had consolidated the combs, and remained on the brood. They were then provided with fragments of combs, where other workers had stored up farina; and to be able to observe what they did with it, we took this substance from some of their cells, and spread it on the board of the hive.

The bees soon discovered both the farina in the combs and what we had exposed. They crowded to the cells, and also descending to the bottom of the hives, took the pollen grain by grain in their teeth, and conveyed it to their mouths. Those that had ate it most greedily mounted the combs before the rest, and stopping on the cells of the young worms, inserted the head, and remained there for a certain time. Burnens opened one of the divisions of the hive gently, and powdered the workers for the purpose of recognising them when they should ascend the combs. He observed them during several hours, and by this means ascertained that they took so great a quantity of pollen only to impart it to their young.

Royal cells were sketched on the 23d. Next day, removing the bees which concealed the brood, we found that all the young worms had jelly as in the ordinary hives, that they had grown and had advanced in their cells, and that others had been lately closed up, probably from the period of their metamorphosis approaching. Finally, from observing the royal cells prolonged, we could not doubt the restoration of order.

Then withdrawing the portions of comb which had been placed by us on the board of the hive, we saw that the pollen had been sensibly diminished in quantity. They were returned to the bees, to augment their provision still farther, for the purpose of extending the experiment. The royal, as well as several common cells were soon closed; and on opening the hive, all the worms were found to have prospered. Some still had their food before them; the cells of others that had spun were shut with a waxen covering.

This result was already very striking; but it particularly excited our astonishment, that notwithstanding such long protracted captivity, the bees did not now seem to have any desire to go out. That agitation, that increasing and periodical disorder, and that common impatience manifested in the first part of the experiment, had ceased. Several bees, indeed, attempted to escape in the course of the day, but finding it impossible, they returned peaceably to their young.

We witnessed these facts repeatedly, and always with equal interest. They so decisively prove the regard of the bees towards the larvæ which they are entrusted with rearing, that we shall not seek for any other explanation of their conduct.

Another fact, no less extraordinary, and much more difficult to be accounted for, was exhibited by bees constrained to work in wax, several times successively, from the syrup of sugar. Towards the close of the experiment they ceased to feed the young, though in the beginning these had received the usual attention. They even frequently dragged them from their cells, and carried them out of the hive.

Ignorant to what this disposition should be ascribed, I endeavoured to revive their instinct, by supplying other brood for their care. My attempts were abortive, for, notwithstanding their stores of farina, they did not feed the young larvæ. We offered them honey, as a more congenial means of alimenting the brood, but also in vain, for the whole perished. Perhaps the bees were incapable of longer producing that paste which is the subsistence of larvæ. Except in this particular, they seemed to have lost none of their faculties—they were alike active and laborious. At last, influenced by motives unknown to us, one day all deserted their hive together, and did not return.

Whatever is the cause affecting the instinct of bees fed too long on honey, perhaps we should consider, with admiration, that this substance undergoes such a modification in flowers, that bees can consume it without inconvenience. But every thing in Nature is adapted for long continued use, and alimentary substances are combined with so much foresight, that their action never proceeds from isolated and unabated energies,

CHAPTER VIII.

ON THE ARCHITECTURE OF BEES.

It is time now to examine how bees convert the substance exuding from their rings to use, and to investigate the treatment changing it to real wax, for it does not leave the receptacles wherein it is moulded in a perfect state. Likewise, it differs there in several respects from its condition after having been wrought. Fusibility alone indicates it to be wax—it is short and friable, wanting that ductility subsequently acquired, and at first it is transparent as plates of talc, whereas it appears opaque and of a whitish yellow in the cells.

Perhaps it may be supposed that bees are provided with instruments corresponding to the angles of their cells. These can be only their teeth or mandibles, their feet, and antennæ; yet there is no more resemblance between the form of the teeth and the angles of the cell, than between the chisel of the sculptor and the work quitting his hands. Plate III. fig. 17, 18, 19. Nor will the figure of the head afford a better explanation. The limbs are fashioned like those of most insects, consisting of the haunch, fig. 20, a; of the thigh, b; the leg, c; and the foot or tarsus, d, e. There is a hollow part in the leg of the third pair, called the basket by Reaumur, wherein bees deposit the farina—fig. 20, 21, c—triangular, smooth, with a row of hairs on the outside, resembling a kind of basket by their inclination. The first articulation of the tarsus is much larger than the rest, and in all the three pairs is of a very different form from those of other insects of the same genus—fig. 20, 21, d. Being employed in collecting the globules of farina scattered over the body of the bee, this articulation is called the brush. That of the third pair of limbs offers some remarkable peculiarities in the prolongation of the lower part, and in the mode of its junction with the leg or pallet, by the figure of which, and its own incurvature, a pair of real pincers is formed. A row of shelly teeth, like those of a comb, proceed from the lower edge of the pallet,—fig. 22, a—corresponding to bundles of very strong hairs, with which the neighbouring portion of the brush is provided. When the two sides of the pincers meet, that is, the under edge of the pallet, and the upper edge of the brush, the hairs of each are incorporated together.

This organization is too obvious, not to have a particular end. Nothing resembling it appears on the limbs of males or queens; but it is seen in humble bees, a race very near to the honey bee, and whose habits bear some analogy.

Farther, the tarsus is composed of three small conical articulations, together with a fourth, which is greatly elongated, and terminates in two pair of claws. Perhaps Reaumur is right in considering this last as being truly two articulations.

The antennæ of bees consist of twelve articulations, the first two forming a peculiar section, move in every direction on the base, and serve to support the next division, composed of the other ten. The first articulation of the whole is globular; the second cylindrical, and much elongated; the third, which commences the second division, is very short and conical; the second very long and conical; the remainder cylindrical; and the last terminated by a soft point. Thus the flexibility of the antennæ enables them to follow the outline of every object.

But the formation of the teeth, limbs, and antennæ cannot, in any respect, explain the structure of the cells, though they can be employed in it; and their effect depends entirely on the object which the insect proposes. Neither does the figure of the waxy scales correspond with the form of the different parts of the cells they are appropriated to build. Hunter inferred that the bottom was nearly equal in thickness to one of them, and that they were accumulated in fashioning the sides of the cells.

Some persons may imagine, perhaps, that glass hives of four sides are sufficiently adapted for exposing the construction of combs; but their architecture is always concealed from our view by clusters of bees, amidst which, and in darkness, the work goes on.

Having taken a large bell-shaped glass receiver, I glued thin wooden slips to the arch at certain intervals, because the glass itself was too smooth to admit of the bees supporting themselves on it. A swarm, consisting of some thousand workers, several hundred males, and a fertile queen, was introduced, and they soon ascended to the top. Those first gaining the slips fixed themselves there by the fore feet; others scrambling up the sides, joined them, by holding their legs with their own, and they thus formed a kind of chain, fastened by the two ends to the upper parts of the receiver, and served as ladders or a bridge to the workers enlarging their number. The latter were united in a cluster, hanging like an inverted pyramid from the top to the bottom of the hive.

The country then affording little honey, we provided the bees with syrup of sugar, in order to hasten their labour. They crowded to the edge of a manger containing it, and having satisfied themselves, returned to the group. We were now struck with the absolute repose of this hive, contrasted with the usual agitation of bees. All the external stratum of the cluster constituted a kind of curtain formed exclusively of wax workers united together, and arranged so as to represent a series of festoons intersecting each other in every direction. The fluctuation of the interior alone communicated motion to this cluster, where the back of the bees generally was opposed to the observer.

Meanwhile, the small bees alone went to forage in the country; they returned with farina, kept guard at the entrance of the hive, cleansed it, and stopped up its edges with the odoriferous resin propolis. The wax workers remained motionless above fifteen hours; the curtain consisting always of the same individuals, assured us that none replaced them. Some hours later we remarked that almost all these individuals had scales under the rings; and next day this phenomenon was still more general. The bees forming the external stratum of the cluster, now having somewhat altered their position, enabled us to see the under part of the abdomen distinctly. By the projection of the scales the rings seemed edged with white; the curtain was rent in several places; less tranquillity reigned in the hive.

Convinced that the combs would originate in the centre of the swarm, our whole attention was then directed towards the vault of the receiver. A worker at this time detached itself from one of the central festoons of the cluster, separated from the crowd, and with its head drove away the bees at the beginning of the row in the middle of the arch. Here turning round, it formed a space for its free motion, an inch or more in diameter, and fixed itself in the centre.*

The worker now employing the pincers at the joint of one of the third pair of limbs, seized a scale projecting from a ring, and brought it forward to the mouth with the claws of the fore legs, where it appeared in a vertical position. Plate IV. fig. 23, 24.

We remarked that the claws turned it in every necessary direction; that the edge of the scale was immediately broke down, and the fragments having been accumulated in the hollow of the teeth, issued forth like a very narrow ribbon, impregnated with a frothy liquid by the tongue. The tongue itself assumed the most varied shape, and executed the most complicated operations; and after imbuing the whole substance of the ribbon, pushed it forward, when it was drawn out a second time, but in an opposite direction.

At length, the bee applied these particles of wax to the vault of the hive where the gluten impregnating them promoted their adhesion, and also communicated a whiteness and opacity, which were wanting when the scales left the rings. Doubtless, this process was

* This description is not quite intelligible.—*T.*

to give the wax that ductility and tenacity belonging to its perfect state. The bee then separated those portions, not yet applied to use, with its teeth, and with the same organs afterwards arranged them at pleasure.

The founder, a name appropriate to this insect, continued the like manœuvre until all the fragments, worked up and impregnated with the fluid, were attached to the vault; when it repeated the preceding operations on the part of the scale yet kept apart, and united what was obtained from it anew to the rest.

A second and third scale were treated thus by the same bee; yet the work was only sketched, for the worker did nothing but accumulate the particles of wax together.

Meanwhile, the founder, quitting its position, disappeared amidst its companions. Another with wax under the rings succeeded it, which, suspending itself to the same spot, withdrew a scale by the pincers of the hind legs, and, passing it through the teeth, prosecuted the work. Observing to make its deposit in a line with the former, it united their extremities. A third worker, detaching itself from the interior of the cluster, now came and reduced some of the scales to paste, and put them near the materials accumulated by its companions, but not in a straight line. One, apparently sensible of the defect, removed the misplaced wax before our eyes, and carrying it to the former heap, deposited it there exactly in the order and direction pointed out. From all these operations was produced a block of rugged surface, depending from the arch, without any perceptible angle, or any traces of cells. It was a simple partition running in a straight line, and without the least inflection, two-thirds of an inch in length, about two-thirds of a cell high, and declining towards the extremities. We have seen other blocks from an inch to an inch and a half long—the form always the same, but none ever of greater height.

The vacuity in the centre of the cluster had permitted us to discover the first manœuvres of the bees, and the art with which they laid the foundations of their edifice. However, it was filled up too soon for our satisfaction, for workers collecting on both faces of the block obstructed our view of their farther operations.

But although unable to behold all that we might desire, we concluded ourselves very fortunate in having an opportunity of doing justice to Reaumur, who thought he observed the wax discharged from the mouth of bees as a paste. Unquestionably he supposed the whitish frothy matter, with which the waxy substance is moistened, to be wax.

Construction of cells.—The cells of bees consist of two parts, a prismatic hexagonal tube, and a pyramidal bottom. The latter, which must be considered the most delicate and essential part of the work, is composed of three equal lozenges, similar, uniting in a common centre, and forming a slight cavity by their reciprocal inclination.

Their depression into one face of the comb makes a projection on the other, there corresponding to three cells partially common to the whole.

It is evident that the hexagonal figure of cells admits of their application by only one angle to the surface of the roof, where many are ranged laterally, and that there must be large vacuities between the angles. But a more solid fixture becomes the marked solicitude of Nature, at two epochs:—1. On the formation of combs. 2. When these have become too heavy to be trusted to so fragile a support.

The first row of cells, that by which the whole comb is attached to the roof of the hive, differs from all the rest. Instead of a hexagon, the orifice is an irregular pentagon. Plate IV., fig. 25, 26, 27. The cell consists of four sides, with the roof of the hive in place of a fifth. The bottom also is different from that of the common cells, consisting of three pieces on the face of the comb, and on the other side of two. Only one of these pieces is a lozenge, fig. 27, 28, c; the other two are of an irregular quadrilateral figure, a, b. On the opposite side of the comb, the bottom of the cells of the first row is composed only of two quadrilateral pieces—fig. 26, b, 29, a, b. Those of faces are seen in front and reverse, fig. 30, 31.*

By the simple dispositions preserved here, the stability of the comb is completely insured, for it touches the interior surface of support in the hive in the greatest possible number of points.

The innumerable crowd accumulated where the labours of bees are conducted obstructs the view of the observer, whence it appeared to me, that the only method of isolating the architects would be inducing them to change the direction of their operations, and work upwards.

* It will be observed, on referring to the engravings, that some of the figures are magnified in explaining the construction of the cells.—*T.*

I had a box made twelve inches square and nine deep, with a moveable glass lid—Plate I., fig. 5, a, b. Combs full of brood honey and farina were next selected from one of my leaf hives, as containing what might interest the bees, and being cut into pieces a foot long, and four inches deep, they were arranged vertically at the bottom of the box, at the same intervals as the insects themselves usually leave between them. A small slip of wooden lath covered the upper edge of each. It was not probable that the bees would attempt to found new combs on the glass roof of the box, because its smoothness precluded the swarm from adhering to it; therefore, if disposed to build, they could do so over the slips resting on the combs, which left a vacuity five inches high above them.

But had it not been for the assistance of Burnens, to whose skill, courage, and assiduity I have been so much indebted in these difficult observations, my invention would have proved of little avail. The appearance of the objects was affected by the glass interposed, and, spite of my remonstrances and the imminent danger, he resolved to remove it. The gentleness of his motions, and the habit of repressing his respiration, could alone preserve him from the wrath of such formidable insects.

As we had foreseen, the swarm with which this box was peopled established itself among the combs below. We then observed the small-bellied bees displaying their natural activity. They dispersed themselves throughout the hive to feed the young larvæ, to clear out their lodgment, and adapt it for their convenience. Certainly the combs, which were roughly cut to fit the bottom of the box, and in some parts damaged, appeared to them shapeless and misplaced, for they speedily commenced their reparation. They beat down the old wax, kneaded it between their teeth, and formed binding to consolidate them. We were astonished beyond expression by such a multitude of workers employed at once in labours to which it did not appear they should have been called, at their coincidence, their zeal, and prudence.

But it was still more wonderful, that about half the numerous population took no part in the proceedings, remaining motionless, while the others fulfilled the functions required. The wax workers, in a state of absolute repose, recalled our former observations. Gorged with the honey we had put within their reach, and continuing in this condition during twenty-four hours, they had secreted that substance so long believed to be collected from the anthers of flowers. The wax, formed under their rings, was now ready to be put in operation, and, to our great satisfaction, we saw a little block rising on one of the slips that we had prepared to receive the superstructure.

No obstacle was offered to the progress of our observations; and, for the second time, we beheld both the undertaking of the founder, and the successive labours of several wax workers, in forming the block. Would that my readers could share the interest which the view of these architects inspired!

The block, originally very small, was enlarged as the work required; and here they excavated a hollow on one side, of about the width of a common cell, and on the opposite surface two others somewhat more elongated. The middle of the single cell corresponded exactly to the partition separating the latter—fig. 32,

33. The arches of these excavations, projecting by the accumulation of wax, were converted into rectilinear prominences, whence the cells of the first row were pentagonal, considering the slip as one side, and those of the second row hexagonal—fig. 34, 35.

The interior conformation of the cavities apparently was derived from the position of their respective outlines. It seemed that the bees, endowed with an admirable delicacy of feeling, directed their teeth principally to the place where the wax was thickest—that is, the parts where other workers on the opposite side had accumulated it; and this explains why the bottom of the cell is excavated in an angular direction behind the projection on the sides of which the sides of the corresponding cells are to rise.

The largest of the excavations, which was opposite to three others, was divided into three parts, while the excavations of the first row on the other face applied against this one were composed of only two.

In consequence of the manner in which the excavations were opposed to each other, those of the second row, and all subsequent, partially applied to three cavities, were composed of three equal lozenges, as the figures explain. I may here remark, that each part of the labour of bees appears the natural result of what has preceded it, therefore chance has no share in these admirable combinations. But let us enter into farther detail.

First row of cells.—A block rose above the slip like a minute vertical partition, five or six lines long, two lines high, but only half a line in thickness;* the edge

* The reader is reminded that all the measurements for this work are originally stated in lines, of which there are twelve in an inch.—*T.*

circular, and the surface rough. Plate IV. fig. 36, 37. Quitting the cluster among the combs, a small bee mounted the slip, turned around the block, and visiting both sides, began to work actively in the middle. It removed as much wax with its teeth as might equal the diameter of a common cell, and after kneading and moistening the particles, deposited them on the edge of the excavation. This insect, having laboured some seconds, retired, and was soon replaced by another. A third continued the work, raising the margin of the edges now projecting from the cavity, and with assistance of its teeth and feet fixing the particles, so as to give these edges a straighter form. More than twenty bees successively participated in the same work; and when the cavity was little above a line and a half in height, though equalling a cell in width, a bee left the swarm, and after encircling the block, commenced its operations on the opposite face, where yet untouched. But its teeth acting only on one-half of this side, the hollow which it formed was opposite to only one of the slight prominences bordering the first cavity. Nearly at the same time another worker began on the right of the face that had been untouched, wherein both were occupied in forming cavities, which may be designed the second and third, and they also were replaced by substitutes. These two latter cavities were separated only by the common margin, framed of particles of wax withdrawn from them, which margin corresponded with the centre of the cavity on the opposite surface. The block itself was still of insufficient dimensions to admit the full diameter of a cell; but while the excavations were deepened, wax workers, extracting their scales, applied them in enlarging its circumference, so that it rose nearly two lines farther around the circular arch. The small bees, which appeared more especially charged with sculpturing the cells, then being enabled to continue their outlines, prolonged the cavities, and heightened their margins on the new addition of wax. Plate V. fig. 38, 39.

Next, the arch, formed by the edge of each of these cavities, was divided as by two equal chords, in the line of which the bees formed stays or projecting borders or margins meeting at an obtuse angle—fig. 40, f, e, b, ; 41, i, b, c,—c, f, l. The cavities now had four margins, two lateral and perpendicular to the supporting slip, and two oblique, which were shorter.

Meantime it became more difficult to follow the operations of the bees, from their frequently interposing the head between the eye of the observer and the bottom of the cell, but the partition, whereon their teeth laboured, had become so transparent as to expose what passed on the other side.

The cavities of which we speak formed the bottom of the first three cells; and while the bees engaged were advancing them to perfection, other workers commenced sketching a second row of cells above the first, and partly behind those in front, for in general their labour proceeds by combination. We cannot say, "when bees have finished this cell, they will begin new ones;" but, "while particular workers advance a certain portion, others carry on the adjacent cells." Farther, the work begun on one face of the comb is already the commencement of that which is to follow on the reverse. All this depends on a reciprocal relation, or a mutual connection of the parts, rendering the whole subservient to each other. It is undoubted, therefore, that slight irregularities on the front will affect the form of the cells on the back of the comb.

Second row of cells.—The bottoms of the first row of cells, composed of two trapeziums and a rhomb, we observed to be larger than those of the cells opposed to them, as these consisted of two trapeziums only.* The space between the edge of these back cells and the edge of the block, admitted the bottom of a common cell, fig. 42; but there was not room for a complete bottom above those in front, as the block rose no higher than r, fig. 43. Several bees employed themselves in sketching the bottom of a new cell, in the unwrought portion of the reverse, fig. 42, proceeding in the following manner:—

They first excavated a vertical fluting, f, m, b, p, in the space comprised between the oblique margins f, c, b, c, of two neighbouring cells, fig. 44, and produced margins, by accumulating to right and left the wax extracted from the block. The perpendicular margins, f, m, b, p, formed by the workers, rose exactly above the angles f, b, of the two lower cells, a, g, fig. 44. At this time the height of the block was insufficient for the whole bottom of a cell, and the fluting as yet was terminated by a curved outline, p, r, m, fig. 44. But other bees established two rectilinear margins on the curvature, which, uniting as two chords in the centre of the arch, formed the obtuse angle m, r, p, fig. 42. The cavity was then bordered by six margins of equal length, forming the outline of a cell, but parts of them somewhat more elevated than others.

* The unlearned reader may be apprized that *trapezium* here merely signifies a four-sided figure, and *rhomb*, a lozenge.—*T.*

The bees now occupied themselves in flattening the back of the rhomboidal portion, f, c, b, e, fig. 42, confining it by the traces f, c, b, e, which they had excavated behind the corresponding margins on the opposite face. Thus, this portion was the first and upper part of a pyramidal bottom, and it occupied a third of the surface of the cavity, there being space enough hollowed out within the hexagon for two similar lozenges.

This space, as yet only sketched, remained in that state until the operations on the opposite face of the comb allowed the bees to construct a vertical margin behind the same cell, which could result only from commencing a new cavity on each side of that margin; but it being established, a bee began to excavate the bottom of the space referred to, still rough, and formed a furrow also vertical in the middle, running from the upper angle of the lozenge to the upper angle of the hexagon. The two pieces, resulting from the division, being smoothed, we observed that they constituted two new lozenges, f, e, r, m, and e, r, b, p, equal to the preceding lozenge f, c, b, e. Thus the six margins of the hexagonal outline surrounded three equal sized lozenges, that is, a complete pyramidal bottom.

The first bottom of this kind was constructed on the posterior face of the block; and, during all these operations, cells were sketched in the same manner to right and left. Meantime the wax workers were enlarging the block, which the punctuated line in fig. 43, shows to have been still inadequate during the construction of the posterior cell of the second row, but it was large enough for new cells of this row, when they commenced the anterior hexagonal cell.

From what has been said, it is easy to conceive that, the bottom of the subsequent cells is always commenced between the oblique upper margins of two adjoining cells. Vertical margins are formed above the angle of each, bordering the new cavity to right and left, and the total circumference is completed by other two oblique margins on the upper curvature of the hollow, whereby a hexagon is produced.

The lower portion of these excavations always corresponds with the intermediate margins of the cells on the opposite side of the comb, whereby all on the front, so to design it, have two lozenges below and one above, while the cells of the reverse have one below and two above.

Each of the six margins, environing the pyramidal bottom of a cell, is destined for one of the six sides forming the tubular part of it. At first sight, nothing appears more simple than adding wax to the margins; but from the inequalities occasioned by the shape of the bottom, the bees must accumulate wax on the depressions to bring them to a level. All the edges of the cells then offer a uniform surface from the commencement, and before the tubes have acquired their proper dimensions. But the surface of a new comb is not quite flat, for there is a progressive degradation in the work of bees—the sides of the cells are prolonged in an order corresponding to the completion of the bottom to which the tubes belong, and the length of these tubes is so well regulated, that there is no interruption or conspicuous irregularity among them. Thence the surface of a new comb is lenticular, its thickness always decreasing to the edges, because the latest cells are shorter than the older ones. While the circumference of the comb extends, this figure is preserved; but when the bees have no more room for its enlargement, they make all the cells equal by prolonging the newer to the dimensions of the older. It gains two flat surfaces, which it will constantly retain.

How can we explain the combination reigning throughout the work of bees? How does instinct lead them to give a different form and different dimensions to the bottom of the first row of cells on either face? Or how can those posted on one side of the block determine the space to be excavated for the mutual relation of their bases? They do not visit the two surfaces alternately to compare the respective position of the cavities they are to sketch; but feeling the part about to be hollowed out with the antennæ, they seem by this means sufficiently apprised of the mode of executing a very complicated work, wherein every thing is exactly arranged.

Nothing is excavated without previously applying the antennæ to the part to be sculptured—bees do not trust to their eyes for any of their operations, but amidst darkness, by the aid of their antennæ, they can fashion even those combs which are deservedly regarded as the most admirable work of insects.

Bees, therefore, seem to be regulated in their work by some local circumstance. We have observed that, while sketching the bottom of a cell before there was any upright margin on the reverse, a projection arose by their pressure on the wax still soft and flexible. Sometimes it occasioned a breach of the partition, which was soon repaired; but a slight prominence always remained on the opposite surface, to the right and left of which they placed themselves to begin a new excavation, and they heaped up part of the materials between the two flutings formed by their labour. The prominence, being converted into a real rectilinear margin, becomes a guide for the direction to be followed by the bees in making the vertical furrow of the front cell.

We have often conceived, from the bees observing the direction of the perpendicular margin so accurately as to work the corresponding hollow, that they were aware of the thickness of the partition, from its flexibility, elasticity, or some other property of the wax. Whatever may be the case, it is certain that, without any mechanical means of measuring it, they give the bottom of the cells a uniform thickness.

Perhaps the proceedings of bees may be explained without resorting to extraordinary causes. The length of the cavities, their respective position, and the thickness of the block, once determined, the inclination of the oblique sides of the trapeziums of the first row to which that of the lozenges of the second is subordinate, is established of itself.

Probably the mode of enlarging the block chiefly contributes to establish the relation between the unequal cells of the first row. Its original height determines nearly the vertical diameter of the posterior cells, which is equal to two-thirds of a common cell. But the bottom of the *front* cell cannot be completed without augmenting the block, which, on that account, is extended more than necessary for finishing the front cell, but allowing just sufficient space for the whole bottom of a *posterior* cell of the second row; for the lozenge-making part of it, is already comprised in the interval between the cells formed of trapeziums. Still advancing the block two-thirds of the diameter of a cell, the bees are enabled to construct the bottom of cells of the second row on the *front*, part being previously intercepted between the upper edges of the first cells; but as yet there will not be room for building the third row, nor until the block shall be enlarged anew.

The bees cannot deviate from the rule prescribed, unless particular circumstances alter the bases of their work; for the block is augmented only by a uniform quantity—and what is admirable, this is done by the wax workers, the depositors of its first elements, but which are not endowed with the faculty of sculpturing the cells.

Thus, in sharing the functions of the hive between the wax workers and the other bees, the Author of Nature seems to have distrusted the exclusive operation of instinct.

May not we deduce from the preceding facts, that the geometry, which apparently embellishes the productions of these insects, is rather the necessary result than the principle of their proceedings?

CHAPTER IX.

MODIFICATIONS OF THE ARCHITECTURE OF BEES.

The vulgar commonly believe that sensations and physical necessity exercise an absolute empire over animals. Unquestionably they have a powerful influence; but although it has been maintained, that selfishness is the sole motive of actions, it would be as difficult to explain the conduct of creatures subjected to instinct, by the attractions of pleasure and the dread of suffering exclusively, as it would be unjust to ascribe the virtues of reflecting and reasoning beings to interested views alone. Neither can any theory explain the apparent exceptions from general rules in the great code of Nature, or how animals of restricted faculties can act in some circumstances, as if they could interpret the design of the Legislator.

Every thing connected with the fabrication and use of combs has been skilfully combined. Cells turned downwards, like those of wasps, would not have suited bees, for they have to store up a fluid. Perhaps the figure of these innumerable minute honey-pots, covering both surfaces of the comb, together with the affinity between their contents and wax, retains the liquid from escaping. The combs hang parallel to each other, and are separated by passages but a few lines wide. This position is not one of the least difficult subjects of explanation; nor would it be practicable, did we conceive that their foundation was laid simultaneously by a number of workers. Blocks of wax are not reared here and there at the same time. A single worker deposits the materials in what seems the suitable direction—it departs, another replaces it—the block rises, and the bees sculpture its opposite faces alternately. But scarcely are some rows of cells constructed, when other two blocks similar to the first are established, one on each side, equally removed from it, and parallel. These very soon become small combs, for the bees work with astonishing rapidity; and in a short time we discover other two, constructed in a position parallel to the preceding. All are extended in proportion to their earlier origin—that in the middle, being the farthest advanced, exceeds those parallel to its two faces by some rows of cells, and the latter exceed those following them by a similar quantity. Thus the greater part of both surfaces of the comb are always concealed by those next to them.

I shall not attempt to explain how the bees take such accurate measurements, and know the direction parallel to the original comb. But were they permitted to rear different blocks in the roof of their hive at the same time, it is obvious that the combs neither would be parallel, nor at suitable distances.

It is invariably a single bee that selects and determines the site of the first cavity, which, being established, serves to direct all the ulterior labours. Did each of several workers sketch a cell at once, the symmetry to result from their operations would be endangered, for these insects, strangers to subordination, are subject to no discipline. A great many bees work on the same comb indeed, but they are not guided by a simultaneous impulse, as might be conceived, without studying their proceedings from the outset. The impulse is successive:—A single bee begins each partial operation, and several others substitute their efforts, tending to the same end; each apparently acts individually, either as directed by the bees preceding it, or by the state in which it finds the work which it is to continue. If any thing can presume consent almost unanimous, it is the inaction of the rest of the colony while a single worker proceeds to determine the position of the comb. After others assist it in extending the block they cease; but a single individual of a different profession, if the word may be allowed, comes to sketch the bottom of a cell, the preparation for another kind of work. It is a base or foundation for establishing the whole edifice. A delicate sense of feeling obtains through the partition the situation of the margin of the opposite cavity, which aids the worker in the proper division of the bottom of the new cells. But it is not by means of the margin only that they discover the direction to be followed—we are convinced that they profit by various circumstances to guide themselves in their excavations.

The bee, however, which forms the first cell, is a remarkable exception—it works in a rough mass, and thus has nothing to point out the way. Instinct is its sole conductor. The workers sketching the cavities of the second row, on the contrary, can profit by the previous margins and angles of the same surface, as a ground or point of departure for future operations.

Their work is executed with much less rapidity in ascending, than when prosecuted in the opposite direction. This was very favourable to our observations, otherwise it would have been impossible to follow the detail of their proceedings. Yet the tardiness of the bees here had its inconveniencies—their work was sometimes interrupted during hours—the materials were wanting, or they were not sculptured soon after being deposited, or several blocks arose on the same slip. The operations evidently were relaxed and impeded; and we were enabled to form an accurate opinion of their architecture only from the different small combs established.

To ascertain whether the above described facts succeeded under ordinary circumstances, I constructed a new hive, the top of which consisted of glass and wooden slips or spars alternately. By means of a screw at each end of the slips, any one could be raised so much as to admit of convenient observation, and it might be replaced without deranging the bees. Plate I. fig. 5, b.

Having lodged a swarm here, we allowed sufficient time for commencement of the first cells, when, turning the screws to bring up the work, we beheld cavities similar to those formed by the operations of bees in ascending. Then lowering the slip, they continued, and our observations being renewed in some minutes, we found the work farther advanced. The cells of the two faces were unlike each other—the bottom had vertical trapeziums, but only those in front had a lozenge in the under part. Cells of the second row were then begun, and every thing proceeded on the same principles, and by the same gradations as when the comb was constructed upwards.

It is clear, in my opinion, therefore, that the particular configuration of the first cells of both faces determines invariably the figure of the pyramidal bottom of all the subsequent cells.

When I compelled the bees to work upwards, they usually commenced their combs on the flat part of the slips. But they were not always so docile, and repeatedly extended old combs in the place where I wished them to build new ones.

If a comb be put under a spar, and the bees work upwards, they begin by lengthening the cells, so as to project a little beyond the spar, and then go on with others on the vertical surface or side of the spar; but the bottoms are flat, being the spar itself. On gaining the upper edge, they extend the extremities of the last constructed cells to the middle of the horizontal surface of the spar, where they raise a block, and sculpture it after the prolongation of these cells. They now give the shape peculiar to the bottom of the foundation row to that of the first which they establish here, and three lozenges to the bottom of the subsequent cells—fig. 45.*

Bees can thus form cells on wood of an hexagonal figure, without having a pyramidal bottom, and perpendicular margins opposite to guide them. But although these flat bottomed cells are less regular than the common cells, and have somewhat angular margins, an hexagonal character is always discernible.

* The author's description is exceedingly obscure.—*T.*

Having seen bees work both up and down, it was natural to investigate whether we could compel them to construct their combs in any other direction. We tried to confound them with a hive glazed above and below, so that they had no place of support but the upright sides of their dwelling. Lodging themselves in the upper angle, they built their combs perpendicular to one of these sides, and as regularly as those which they usually build under a horizontal surface. The foundations were laid on a place which does not serve naturally for the base; yet, except in the difference of direction, the first row of cells resembled those in ordinary hives. The others were no less fit for use, distributed on both faces, and the bottoms alternately corresponded with the same symmetry.

I put the bees still to a greater trial. As they now testified their inclination to carry their combs, in the shortest way, to the opposite side of the hive, for they prefer uniting them to wood, or a surface rougher than glass, I covered it with a pane. Whenever this smooth and slippery substance was interposed between them and the wood, they departed from the straight line hitherto followed, and bent the structure of their comb at a right angle to what was already made, so that the prolongation of the extremity might reach another side of the hive, which had been left free.

Varying this experiment after several fashions, I saw the bees constantly change the direction of their combs, when I approximated a surface too smooth to admit of their clustering on it. They always sought the wooden sides. I thus compelled them to curve the combs in the strangest shapes, by placing a pane at a certain distance from their edges.

These results indicate a degree of instinct truly wonderful—they denote even more than instinct, for glass is not a substance against which bees can be warned by Nature. In trees, their natural abode, there is nothing that resembles it, or with the same polish. The most singular part of their proceeding is changing the direction of the work before arriving at the surface of the glass, and while yet at a distance suitable for doing so. Do they anticipate the inconvenience which would attend any other mode of building?

No less curious is the plan adopted by the bees for producing an angle in the combs—the wonted fashion of their work, and the dimensions of the cells, must be altered; therefore, the cells on the upper or convex side of the comb are enlarged—they are constructed of three or four times the width of those on the opposite surface. How can so many insects, occupied at once on the edges of the combs, concur in giving them a common curvature from one extremity to the other? How do they resolve on establishing cells so small on one side, while dimensions so enlarged are bestowed on those of the other? And is it not still more singular, that they have the art of making a correspondence between cells of such reciprocal discrepancy? The bottom being common to both, the tubes alone assume a taper form. Perhaps no other insect has afforded a more decisive proof of the resources of instinct, when compelled to deviate from the ordinary course.

But let us study them in their natural state, and there we shall find that the diameter of their cells must be adapted to the individuals which shall be bred in them. The cells of males have the same figure, the same number of lozenges and sides as those of workers, and angles of the same size. Their diameter is three and one-third lines, while those of workers are only two and two-fifths.

It is rarely that the cells of males occupy the higher part of the combs. They are generally in the middle or on the sides, where they are not isolated. The manner in which they are surrounded by other cells alone can explain how the transition in size is effected. When the cells of males are to be fabricated under those of workers the bees make several rows of intermediate cells, whose diameter augments progressively, until gaining that proportion proper to the cells required ; and in returning to those of workers a degradation is observed in a manner corresponding.

Usually there are three or four rows of intermediate cells. The first of those for males still participate in the irregularity of the adjoining margins, the bottoms there corresponding to four cells instead of three. Their furrows are always in the direction of the margins, but on one face the side of the cell, instead of being immediately opposite to the centre of the cell on the reverse, divides it unequally, which alters the shape of the bottom, so that it no longer contains three uniform lozenges, but consists of pieces more or less irregular.*

The farther removed from the transition cells, those of the males become more regular. Several rows are free of any defect. But the irregularity is resumed

* This part of the subject cannot be easily understood, without actually inspecting combs and attending to the subsequent explanations.—*T.*

on their opposite confines, nor does it disappear until several rows of workers' cells, of an extraordinary fashion, are interposed.

Bees, in preparing the cells of males, previously establish a block or lump of wax on the edge of their comb, thicker than is usually employed for those of workers. It is also made higher, otherwise the same order and symmetry could not be preserved on a larger scale.

Several naturalists notice the irregularities in the cells of bees as so many defects. What would have been their astonishment had they observed that part of them are the result of calculation. Had they followed the imperfection of their organs, some other means of compensating them would have been granted to the insects. It is much more surprising that they know to quit the ordinary route, when circumstances demand the construction of enlarged cells; and after building thirty or forty rows of them, to return to the proper proportions from which they have departed, by successive reductions.

It appears that the particular species of cells to be constructed by bees is determined by the laying of the queen. They never build the cells of males so long as she produces the eggs of workers; but when she wants a place for depositation of the former, they very soon become sensible of it—they are seen forming their cells irregularly, gradually giving them greater diameter, and finally preparing a cradle for reception of the whole masculine race.

There is another circumstance under which bees augment the dimensions of their cells, namely, when there is an opportunity for a great collection of honey.

Not only are they then constructed of a diameter much exceeding that of the common cells, but they are elongated throughout the whole space admitting it. A great portion of irregular comb contains cells an inch, or even an inch and a half in depth.

Bees, on the contrary, sometimes are induced to shorten their cells. When wishing to prolong an old comb, whose cells have received their full dimensions, they gradually reduce the thickness of its edges, by gnawing down the sides of the cells, until restoring it to its original lenticular form. They add a waxen block around the whole circumference, and on the edge of the comb construct pyramidal bottoms, such as those fabricated on ordinary occasions. It is a certain fact, that a comb never is extended in any direction unless the bees have thinned the edges, which are diminished throughout a sufficient space to remove any angular projection.

The law which obliges these insects partly to demolish the cells on the edges of the comb before enlarging it, unquestionably demands more profound investigation. How can we account for instinct leading them to undo what they have executed with the utmost care? The wonted regular gradation, which may be necessary for new cells, subsists among those adjoining the edges of a comb recently constructed. But afterwards, when those on the edge are deepened like the cells of the rest of the surface, the bees no longer preserve the decreasing gradation which is seen in the new combs. Thus, it is evidently for the purpose of restoring the comb to its primitive form, which prepares the circumference for enlargment, that they reduce the depth of the cells proportionally to their distance from the edge.

All the anomalies exhibited in the labours of bees are so well appropriated to the object proposed, that they seem to constitute part of the plan under which the creatures act, and which concur for the general order.

APPENDIX,

BY P. HUBER,

TO THE OBSERVATIONS ON THE ARCHITECTURE OF BEES.

THE original foundation of a comb comprehends three or four cells—sometimes more; and after being continued of that breadth for two or three inches, it begins at about three-fourths of this length to be enlarged.

Were the work prosecuted only in descending, it would form a narrow stripe, and advance slowly. But it is necessary that it should proceed rapidly, and that numbers of bees should be able to operate in all directions. The more the comb is enlarged below, the more essential that it should rise immediately to reach the vault of the hive.

It thence results, that all the cells of the first row are not constructed at once throughout the hive. Those alone can be considered primitive which are built before the broadening of the comb. Nevertheless, the cells of that row formed either by ascending or obliquely have nearly the same shape as the primitive cells; likewise, though there be somewhat greater confusion and irregularity, the general solidity suffers nothing.

Bees work in all directions; in every case their proceedings are uniform. Meanwhile we should be unable to recognise the little original waxen block, without adverting, that it appears like a ribband two or three lines broad, running round the edge of the combs, and seemingly more compact than the rest. In this edge the bees sculpture their new cells; and here they deposit their scales of wax, labouring over the whole circumference at the same time, when they have abundance of that substance.

Although the work goes on in all points, it does not advance in the same progression. Bees build quicker downwards than horizontally, and slowest in ascending. Thence ensues the kind of lens or elliptic shape of the comb at the period of its enlargement; thence also the greater length than breadth—its being more pointed at the lower extremity, and narrower towards the top than in the middle. The shape of the combs, therefore, is very regular—their outline in general is void of any asperity; besides, a singular harmony subsists among all the cells in their elongation. We have previously stated, that this is in proportion to their priority; but investigating the fact with greater attention, we have observed that in a new comb their length is proportioned to their distance from the edge. Thus the first row of cells is not the deepest—the cells are there shallower than those in the middle of the comb, but when the comb acquires a certain weight, the bees hasten to prolong those so essential to the solidity of the whole, sometimes making them deeper than those which follow.

The cells are not perfectly horizontal, the orifice being almost always a little higher than the bottom, which enables us to ascertain the original position of a comb, though detached. Hence the axis of a cell is not perpendicular to the partition separating the two faces of the comb. This fact, which has been hitherto overlooked, is an insuperable bar to geometrical calculations regarding the figure of cells, because they have a certain inclination from the base. Sometimes they deviate from the level of the horizon above 20 degrees, and commonly 4 or 5.

Yet, whatever be their irregularities here, they are less conspicuous than those of the bottom; and frequently where these last are irregular, the tubes preserve a hexagonal shape.

Bees in general observe a tendency to symmetry, not so much perhaps in small details as in the totality of their operations. Sometimes, indeed, the combs exhibit a singular formation, but on following all the minutiæ of labour, we can, for the most part, assign reasons for the apparent anomalies. These insects are obliged to adapt themselves to localities—one irregularity produces another, and it usually originates in the arrangements which we make them adopt. The inconstancy of the temperature of the atmosphere affects the symmetry of the combs, from frequently interrupting the operations of those bees entrusted with their construction. We have remarked that less perfection is offered by a work resumed than one of uninterrupted labour.

On allowing too little interval between the spars for receiving the foundation of the combs, the work has assumed a particular direction. At first the bees did not seem sensible of the defect, but very soon they appeared to suspect their error, and gradually changing the line of the work, they gained the customary distances. This having given the comb a recurvature, new ones commenced opposite to its middle necessarily had the like deformity, which was imparted to all the others successively. Meantime, however, the bees endeavoured as much as possible to bring them back to the regular form. Often a comb is convex above, farther down the defect is rectified, and the surfaces of the lower part are brought to the proper figure.

We have seen their regard for symmetry exhibited in a manner still more decided. From a series of preceding irregularities, having been induced to plant two blocks instead of one on a spar, but not in the same line, they could neither be enlarged without interrupting each other, nor could their edges unite, from their respective position. However, the bees adopted a very effectual plan—they curved the edges of the two combs, and brought them to meet so perfectly that they could continue them together. The part above this junction diverged greatly from a straight line, but in proportion as the united combs were prolonged, their surface became more and more level, and at length perfectly uniform.

A comb never commences with the cells of males, the first rows being formed of very regular small cells; but the reciprocal correspondence among them soon ceases to be so exact, and the bottoms are less symmetrical. It would be impossible for the bees to produce an absolute correspondence between unequal cells and those perfectly regular—we frequently observe little masses of wax occupying the intervals between them. By thickening the sides and rendering the circumference more circular they sometimes succeed in uniting cells of different diameter, for they have more than one fashion of compensating irregularities.

The modifications of the bottom of cells are so much more decided and constant as to announce a determinate plan, and also explains their progressive enlargement. Examining a comb by a vertical line from the beginning, and passing through the middle, we observe that the cells next to this line enlarge without much alteration of shape; but the bottom of those adjacent no longer consists of three equal lozenges. Each, instead of corresponding with three others, corresponds with four cells on the opposite face, while their orifices are not the less hexagonal. The bottom is composed of four pieces, two of which are hexagonal, and two rhomboidal or four-sided, whose size and figure vary. These cells, which are somewhat larger than the third part of three opposite cells, comprehend a portion of the bottom of a fourth cell in their circumference. Under the last regular pyramidal bottoms are found those having a bottom consisting of four parts, three very large and one a rhomb, very small. A great interval separates the two rhombs of the cells of transition, while the two hexagonal portions adjoin and are perfectly similar—Plate V., fig. 46, 47. There is less inequality between the two rhombs of the bottom a cell farther down—its circumference embraces more of the fourth cell on the opposite surface. Finally, we find a great number of cells whose bottom consists of four pieces perfectly regular—namely, two elongated hexagons, and two lozenges equal, but smaller than those of the pyramidal bottom, fig. 48. Proportionally as we recede from cells whose bottoms contain regular four-sided portions, whether by descending or proceeding from right to left, we observe the cells recovering their ordinary form—that is, one of the lozenges contracts and at length disappears entirely—fig. 49, 50. The pyramidal shape of the bottom is resumed, but of larger dimensions than in the cells of the higher part of the comb. It is preserved with the utmost regularity through many rows; afterwards the cells are altered, and we remark the presence of four pieces in the bottom until the common cells are restored.

Thus it is by encroaching a little on the limits of those of the opposite face that bees come at last to give their cells the largest dimensions. The gradation of the transition cells being reciprocal on both faces of the comb, it follows that the hexagonal circumference on each embraces four cells.

Having reached any degree whatever of this progression, the bees can stop there and preserve it in several consecutive rows. They seem to continue longest at the medium, where we find a great many cells with the bottom quite regularly constructed of four pieces. Therefore, they could build the whole comb on this plan, were not their purpose to resume the pyramidal form they have left. In diminishing the diameter of their cells, bees return by similar gradations, in an inverted order.

It would be very difficult to measure the inclination of the four-pieced bottoms; but they seem a little shallower than those which are pyramidal, and it should be so, for the two lozenges being smaller, the line connecting their extremities is less depressed.

In general, it appears that the form of the tube of the cell is more essential than the shape of the bottom, as we have seen bottoms of four irregular pieces with hexagonal tubes, and cells also of six sides constructed on glass or wood, which served for the bottom instead of wax. The figure of the bottom depends on the intersection of the outline of the cells on both faces, in the direction of the margins.

The shape of the sides of the four-pieced bottoms differs according to the facettes from which they rise—those corresponding to one side of a lozenge and part of a hexagonal facette are bevelled, that they may be adapted to each other, while the two sides corresponding to the long side of the hexagon are rectangular parallelograms.

CHAPTER X.

COMPLETION OF THE CELLS.

THE substance whereof the cells are formed is in its origin of a dull white colour, semi-transparent, soft, and uniform, without being smooth; but in a few days it loses most of these qualities, or rather acquires new ones. A yellowish tint pervades the interior surface of the cells; their edges, now much thicker than at first, are far less regular, and, from being incapable of resisting the slightest pressure, they have attained a consistence of which they did not seem susceptible.

We had remarked the greater proportional weight of finished combs than of those in a progressive state—that the latter break on the slightest touch, whereas the others rather yield than fracture. Their orifices have something glutinous at this stage, and white cells melt at a much lower temperature of water than what liquifies the coloured cells. Old combs, therefore, contain a substance foreign to wax.

In examining the orifices of the yellow cells, we perceived that their circumference was surrounded by a reddish, unctuous, odoriferous varnish, indicating, as we thought, the resin propolis. Afterwards it appeared not to be restricted to the orifices, but that reddish threads of it were applied around all the interior of the sides, like a kind of soldering, at the points of contact of the different pieces, to strengthen them.

Bees are obliged to interrupt their labour when they can procure no more wax, during which time, probably, they varnish the edges of their cells. On resuming their work, the cells are deepened, and traces of the propolis remain pointing out where it has been applied.

In so far as we were aware, these peculiarities had not struck any of the authors on bees; therefore, it was important to ascertain that propolis enters into the composition of cells.

The substance taken from the sides of a hive, and the fragments of cells bordered with it, imparted a golden yellow colour to ether, spirit of wine, and spirit of turpentine. The brown matter of the cells was dissolved there, even when they were cold. In the two latter fluids the orifices still retained the cellular form and their yellow tinge after losing the varnish. This was also lost in ether; and the cells, quickly whitening, disappeared with solution of the wax.

The propolis of the orifices of cells and of the sides of a hive, being softened by gentle heat, could be drawn out in a thread. Nitrous acid, at a low heat, poured on both, whitened the yellow wax in a few minutes; but the varnish of the orifices and masses of propolis underwent no alteration.

When some orifices were put into boiling water, and the wax melting, formed a cake, the varnish remained entire above it, the hexagonal figure of the cells being preserved, while their diameter seemed a little enlarged.

Fixed caustic alkali, which forms a kind of soap with wax, had no effect on the varnish, though cells were immersed in it several months.

The substance colouring the edges of cells and the lines on their sides, therefore, have the greatest analogy to propolis; and the yellow of the cells has no relation to the varnish at the places where the sides unite.

Notwithstanding my confidence in such conclusions, I felt that they would become indisputable, only by surprising the bees in collecting propolis. For many years I had fruitlessly endeavoured to find them on trees producing an analogous substance, though having seen multitudes returning laden with it.

In July, some branches of the wild poplar, which had been cut since spring, with very large buds full of a reddish, viscous, odoriferous matter, were brought to me, and I planted them in vessels before hives in the way of the bees going out to forage, so that they could not be insensible of their presence. Within a quarter of an hour they were visited by a bee, which, separating the involucra of a bud with its teeth, drew out threads of the viscous substance, and lodged a pellet of it in one of the baskets of its limbs. From another bud it collected another pellet for the opposite limb, and departed to the hive. A second bee replaced it in a few minutes, following the same procedure.

Young shoots of poplar, recently cut, did not seem to attract these insects, but their viscous matter had less consistence than the former.

Different experiments proved the identity of this substance with propolis; and now having only to discover how the bees applied it to use, we peopled a hive so prepared as to fulfil our views.

The bees, building upwards, soon reached the glass above; but unable to quit their habitation on account of supervening rains, they were three weeks without bringing home propolis. Their combs remained perfectly white until the beginning of July, when the state of the atmosphere became more favourable for our observations. Serene warm weather engaged them to forage; and they returned from the fields, laden with a resinous gum, resembling a transparent jelly, and having the colour and lustre of the garnet. It was easily distinguished from the farinaceous pellets then collected by other bees. The workers bearing the propolis ran over the clusters suspended from the roof of the hive, and rested on the rods supporting the combs, or sometimes stopped on the sides of their dwelling, in expectation of their companions coming to disencumber them of their burden. We actually saw two or three arrive, and carry the propolis from off the limbs of each with their teeth. The upper part of the hive exhibited the most animated spectacle—thither a multitude of bees resorted from all quarters, to engage in the predominant occupation of the collection, distribution, and application of the propolis. Some conveyed that of which they had unloaded the purveyors in their teeth and deposited it in heaps; others hastened to spread it out, before hardening, like a varnish, or formed it into strings, proportioned to the interstices of the sides of the hive to be filled up. Nothing could be more diversified than the operations carried on.

The bees apparently charged with applying the propolis within the cells, were easily distinguished from the multitude of workers, by the direction of their heads towards the horizontal pane forming the roof of the hive, and, on reaching it, they deposited their burden nearly in the middle of intervals separating the combs. Then they conveyed the propolis to the real place of its destination. They suspended themselves by the claws of the hind legs to points of support, afforded by the viscosity of the propolis on the glass; and as if swinging themselves backwards and forwards, brought the heap of this substance nearer to the cells at each impulse. Here the bees employed their fore feet, remaining free, to sweep up what the teeth had detached, and to unite the fragments scattered over the glass, which recovered all its transparency, when the whole propolis was brought to the vicinity of the cells.

After some of the bees had smoothed down and cleaned out the glazed cells, feeling the way with their antennæ, one desisted, and having approached a heap of propolis, drew out a thread with its teeth. This being broke off, it was taken in the claws of the fore feet, and the bee, re-entering the cell, immediately placed it in the angle of two portions that had been smoothed, in which operation the fore feet and teeth were used alternately. But probably proving too clumsy, the thread was reduced and polished, and we admired the accuracy with which it was adjusted when the work was completed. The insect did not stop here. Returning to the cell, it prepared other parts of it to receive a second thread, for which we did not doubt that the heap would be resorted to. Contrary to our expectation, however, it availed itself of the portion of the thread cut off on the former occasion, arranged it in the appointed place, and gave it all the solidity and finish of which it was susceptible. Other bees concluded the work which the first had begun, and the sides of the cells were speedily secured with threads of propolis, while some were also put on the orifices. But we could not seize the moment when they were varnished, though it may be easily conceived how it is done.

Experiments demonstrate that the matter imparting the yellow colouring to wax has no analogy with propolis. This is not the natural hue of wax, for new cells are white. But the whiteness gives place to a yellow tinge, which afterwards deepens. Sometimes the new combs yellow in two or three days. As other naturalists, I was inclined to ascribe the alteration to the heat of the hives—to the vapours disseminated in them—to the emanations of the honey—or of it and the pollen remaining in the cells; yet these opinions could not support rigorous examination. New combs have been observed to undergo no alteration in several months. Sometimes one face has been of a jonquil colour, and the other white; and on the same surface we have observed a portion consisting of cells of a very lively yellow, while those adjacent had lost none of their whiteness. Likewise a single cell has shown several of the sides yellow, others white on one side, or both white and yellow. Honey and pollen would have tinged all the sides uniformly, as far as they reached; and the vapours disseminated throughout the hive would have had a general influence.

In order to ascertain the effect of preserving the combs from the contact of bees, I took a hive divided in the middle, and kept a piece of perfectly white comb in it during a month, liable to heat, moisture, and all atmospheric vapours, without the colour being affected. Meanwhile, the combs exposed to the contact of the bees yellowed more and more, but this colouring was partial and distributed irregularly, as if in stripes. Every thing argued that it resulted from some direct action on the part of the bees, not from exposure in the interior of hives.

Without actual discovery of the fact, we have been induced to ascribe it to two different sources: first, to the bees rubbing different parts of the body, their teeth or feet on the surfaces where they seem to rest; secondly, to the trunk sweeping from right to left, like a fine, pliant pencil, when it appears to leave some sprinkling of a transparent liquid. We shall not affirm which of these operations is the source of the yellowness of wax, but we incline to refer it to the first, because after the bees had rubbed certain cells with their teeth and fore legs, we sometimes thought the colour altered.

These insects do not restrict themselves to painting and varnishing their cells; they are also occupied in giving greater solidity to the edifice itself, by means of a mortar which they know to compose for that purpose.

The ancients were acquainted with some of the properties of propolis; and aware that the bees, under certain circumstances, mixed it with wax, they gave the name *pissoceros* to the compound; and how well they had studied the subject was proved by one of my experiments.

I immersed some fragments of the compound taken from the sides of an old hive in ether, having already found that this fluid held a very small portion of wax in solution. Decanting it several times, I concluded that the whole propolis was dissolved, when the fluid ceased to colour. Accordingly, the residue was found to consist of a little white wax which the bees had mixed with it.

Pliny believed that these insects used a mixture of wax and propolis in constructing the fixture or basis of the comb; Reaumur, on the contrary, thought it only pure wax. Perhaps the opinion of such eminent naturalists may be reconciled by the following facts:—

Soon after some new combs had been finished in a hive, manifest disorder and agitation prevailed among the bees. They seemed to attack their own works. The primitive cells, whose structure we had admired, scarcely were recognizable. Thick and massy walls, heavy shapeless pillars were substituted for the slight partitions, previously built with such regularity. The substance had changed along with the form, being composed apparently of wax and propolis. From the perseverance of the workers in their devastations, we suspected that they proposed some useful alteration of their edifices; and our attention was directed to the cells least injured. Several were yet untouched; but the bees soon rushed precipitately on them, destroyed the tubes, broke down the wax, and threw off the fragments. But we remarked that the bottom of the cells of the first row was spared; neither were the corresponding parts on both faces of the comb demolished at the same time. The bees laboured at them alternately, leaving some of the natural supports, otherwise the comb would have fallen down, which was not their object; they wished, on the contrary, to provide it a more solid base, and to secure its union to the vault of the hive, with a substance whose adhesive properties infinitely surpassed those of wax.

The propolis employed on this occasion had been deposited in a mass over a cleft of the hive, and had hardened in drying, which probably rendered it more suitable for the purpose. But the bees experienced some difficulty in making any impression on it; and we thought, as also had appeared to M. de Reaumur, that they softened it with the same frothy matter from the tongue which they use to render wax more ductile.

We very distinctly observed the bees mixing fragments of old wax with the propolis, kneading the two substances together to incorporate them; and the compound was employed in rebuilding the cells that had been destroyed. But they did not now follow their ordinary rules of architecture, for they were occupied by the solidity of their edifices alone. Night intervening, suspended our observations, but next morning confirmed what we had seen.

We find, therefore, that there is an epoch in the labour of bees, when the upper foundation of their combs is constructed simply of wax, as Reaumur credited, and that after all the requisite conditions have been attained, it is converted to a mixture of wax and propolis, as remarked by Pliny so many ages before us. Thus is the apparent contradiction between these two great naturalists explained. The first row of cells had been temporarily established as a base, and to serve as a direction for the pyramidal bottoms of those to be subsequently constructed. So long as the magazines were not quite full, this was sufficient to support the edifice; but plates of wax so thin probably would have been inadequate to sustain the weight of several pounds. The bees seemed to have anticipated the eventual inconvenience—they destroyed the frail sides of the first row of cells, to form strong pillars and waxen walls of a viscous and compact substance. Perhaps the alteration depends on the concurrence of special circumstances, nor does it ensue at a marked and regular period. Sometimes the bees only border the sides of the upper cells with propolis, without changing their shape, or strengthening them.

But this is not the utmost extent of the foresight of these insects. When they have plenty of wax, they make their combs the full breadth of the hive, and solder them to the glass or wooden sides, by structures, more or less approaching the form of cells, as circumstances admit. But should the supply of wax fail before they have been able to give sufficient diameter to the combs, whose edges are rounded, large intervals remain between them and the upright sides of the hive, and they are fixed only at the top. Therefore, did not the bees provide against it, by constructing great pieces of wax, mixed with propolis, in the intervals, they might be borne down by the weight of the honey. These pieces are of irregular shape, strangely hollowed out, and their cavities void of symmetry.

During winter, a comb in one of my bell glass hives, having been originally insecure, fell down, but preserving its position parallel to the rest. The bees were unable to fill up the vacuity left above it, because they do not build combs of old wax, and none new could be then obtained. At a more favourable season they would have engrafted a new comb on the old one; but now their provision of honey could not be spared for the elaboration of this substance, which induced them to ensure the stability of the comb by another process.

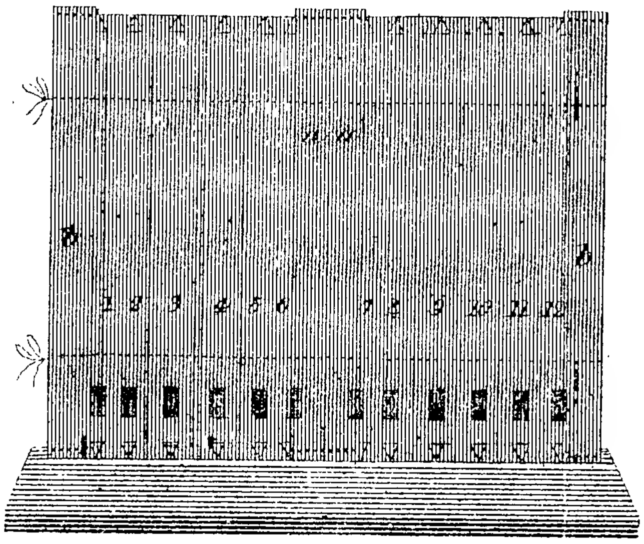
Crowds of bees taking wax from the lower part of other combs, and even gnawing it from the surface of the orifices of the deepest cells, they constructed so many irregular pillars, joists, or buttresses, between the sides of the fallen comb, and the others on the glass of the hive. All these were artificially adapted to localities. Neither did they confine themselves to repairing the accidents which their works had sustained. They seemed to profit by the warning, to guard against a similar casualty.

The remaining combs were not displaced; therefore, while solidly adhering by the base, we were greatly surprised to see the bees strengthen their principal fixtures with old wax. They rendered them much thicker than before, and fabricated a number of new connections, to unite them more firmly to each other, and to the sides of their dwelling. All this passed in the middle of January, a time that these insects commonly keep in the upper part of their hive, and when work is no longer seasonable.

I may restrain myself from reflections and commentaries, but I acknowledge that at such measures I cannot suppress sentiments of admiration.

PLATE I.

1.



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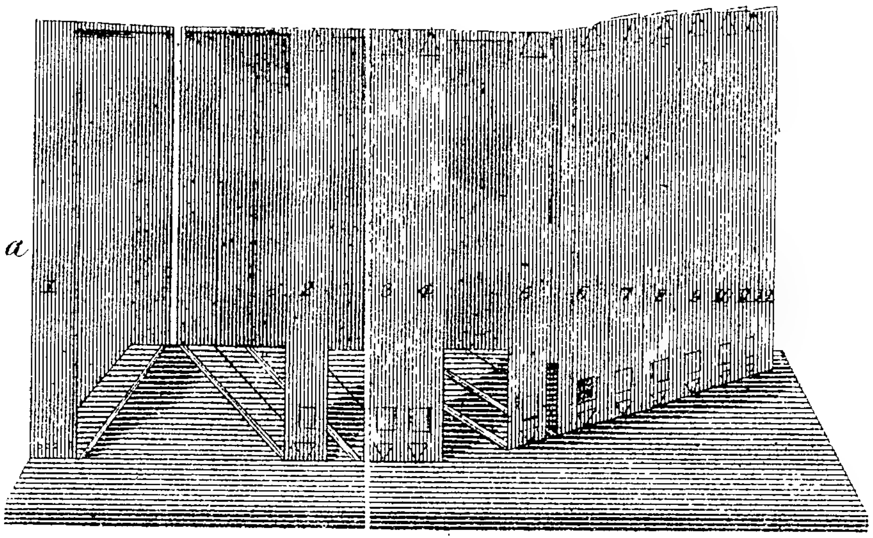

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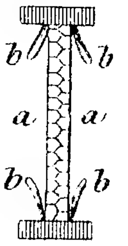

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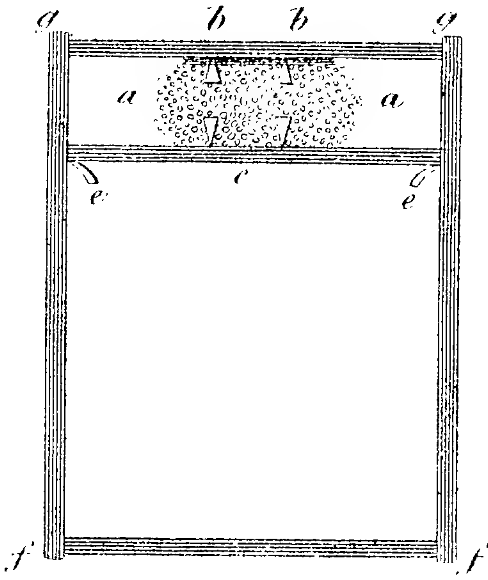

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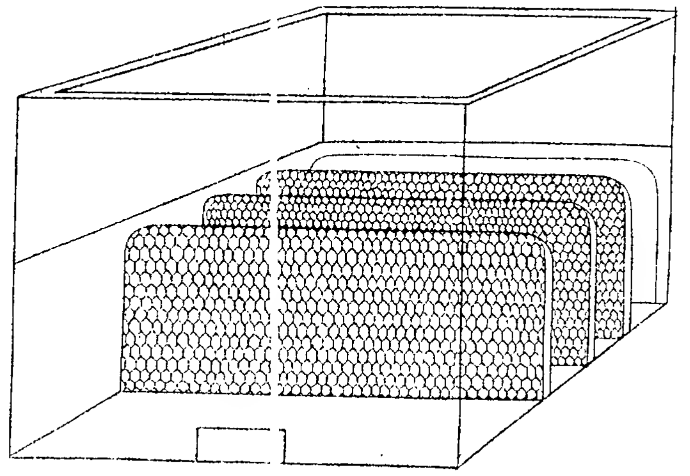

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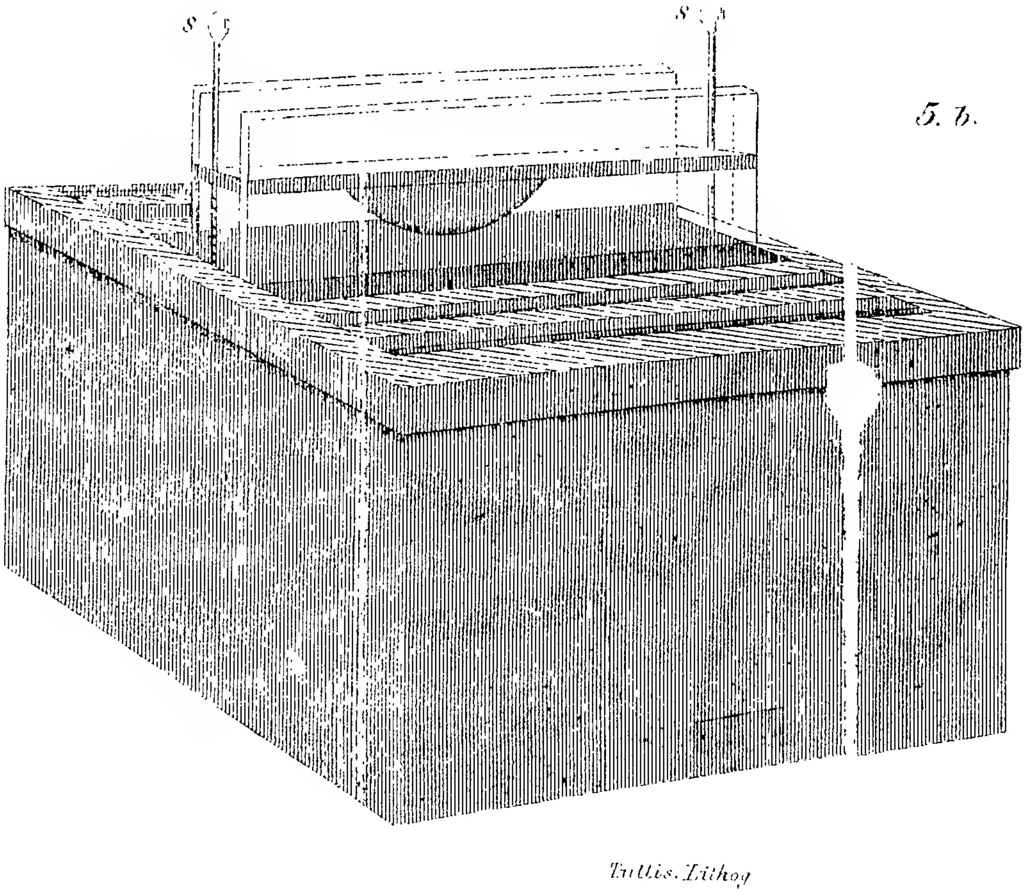
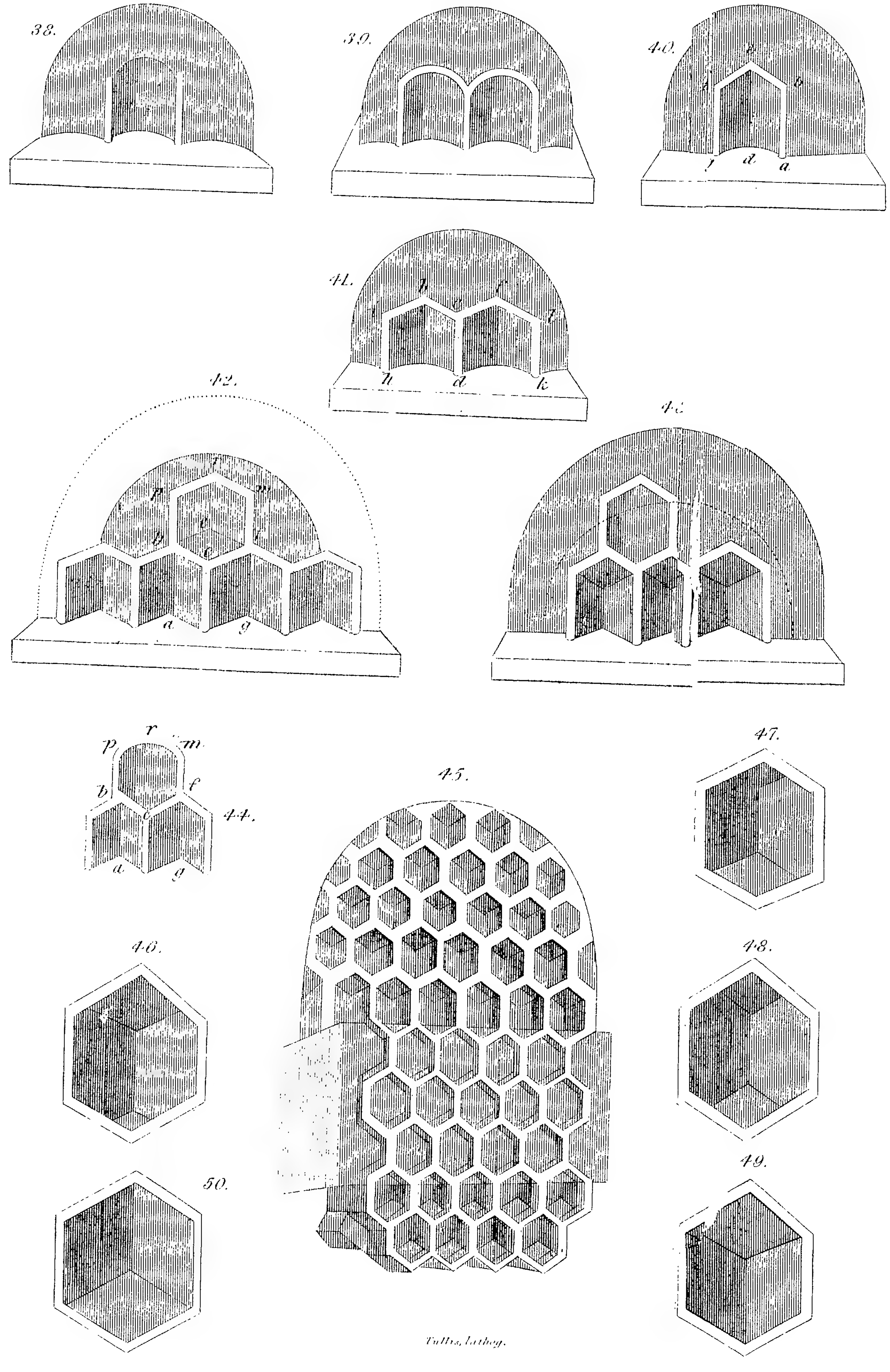


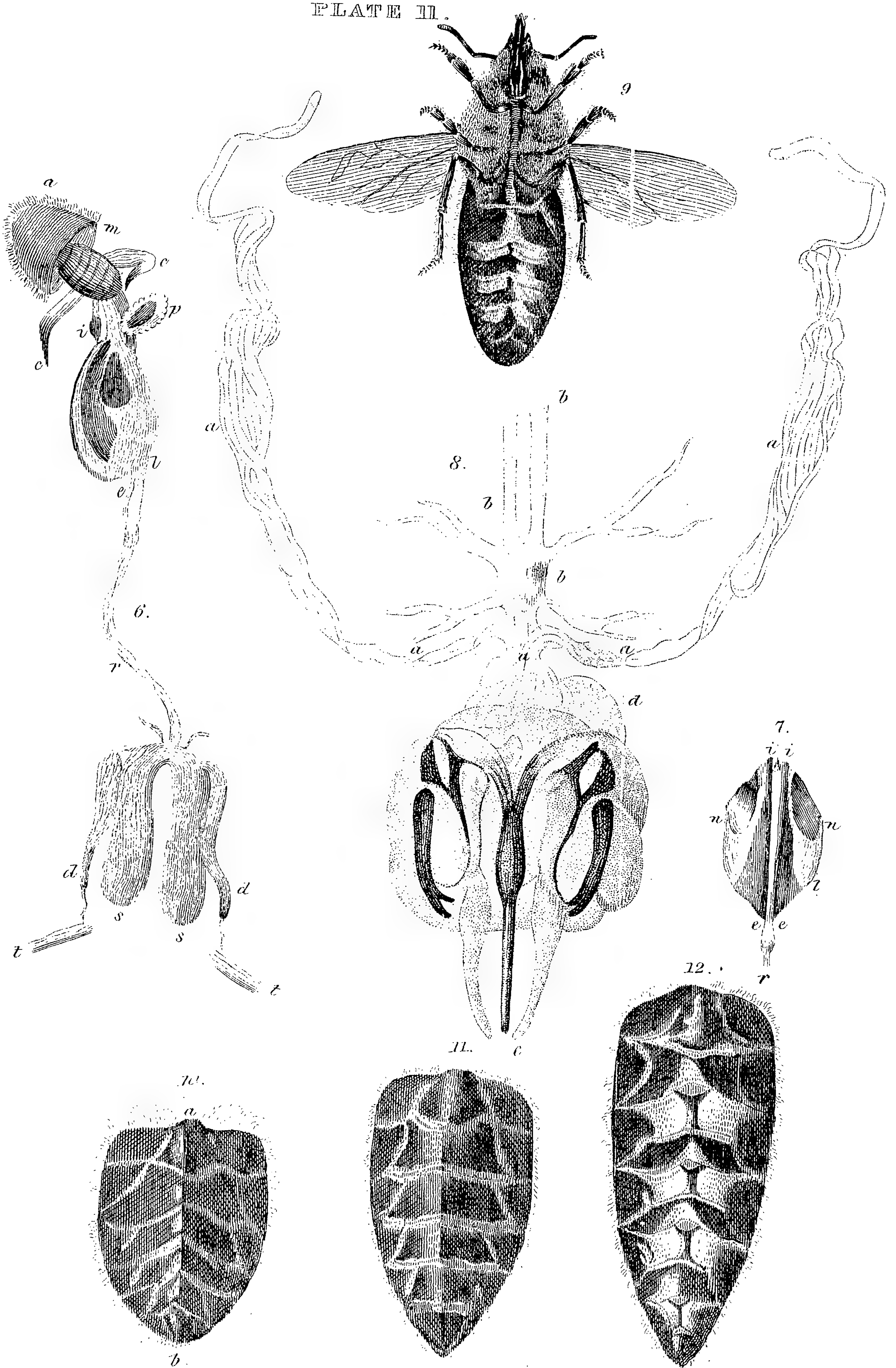
Tullis. Lithog

PLATE V.



Tullis, lithog.

PLATE II.



EXPLANATION

OF THE

VIGNETTE AND PLATES.

Vignette.—In the Vignette which precedes this volume, the reader will find an illustration of the external appearances which characterise the sexes of the Honey-bee. With the aid of these illustrations, and the following description extracted from the Introduction to Entomology, by Kirby and Spence, a very complete idea may be formed of the features which distinguish the inhabitants of the hive. The society of a hive of bees, besides the young brood, consists of one female or queen, several hundreds of males or drones, and many thousand workers; and of these the following are the most distinctive features:

I. *The Female or Queen* is considerably longer than that of either the drone or the worker. The prevailing colour in all three is the same, black or black-brown; but with respect to the female this does not appear to be invariably the case. Reaumur affirms, after describing some differences of colour in different individuals of this sex, that a queen may always be distinguished, both from the workers and males, by the colour of her body. If this observation be restricted to the colour of some parts of her body, it is correct; but it will not apply to all generally; (unless, as I suspect may be the case, by the term body he means the abdomen ;) for, in all that I have had an opportunity of examining, the prevailing colour, as I have stated it, is the same.

The *head* is not larger than that of the workers; but the *tongue* is shorter and more slender, with straighter *maxillæ*. The *mandibles* are forficate, and do not jut out like theirs into a prominent angle; they are of the colour of pitch with a red tinge, and terminate in two teeth, the exterior being acute, and the interior blunt or truncated. The *labrum* or upper-lip is fulvous; and the *antennæ* are piceous.

In the *trunk*, the *tegulæ* or scales that defend the base of the wings are rufo-piceous. The *wings* reach only to the tip of the third abdominal segment. The *tarsi* and the apex of the *tibiæ* are rufo-fulvous. The posterior *tibiæ* are plane above and covered with short adpressed hairs, having neither the *corbicula*, (or marginal fringe of hairs for carrying the masses of pollen,) nor the *pecten;* and the posterior *plantæ* have neither the brush formed of hairs set in striæ, nor the auricle at the base.

The *abdomen* is considerably longer than the head and trunk taken together, receding from the trunk, elongato-conical, and rather sharp at the anus. The *dorsal* segments are fulvous at the tip; covered with very short, pallid, and in certain lengths, shining, adpressed hairs; the first segment being very short, and covered with longer hairs. The *ventral* segments, except the *anal*, which is black, are fulvescent or rufo-fulvous, and covered with soft, longer hairs. The *vagina* of the *spicula* (commonly called the sting) is curved.

II. *The Male Bee, or Drone,* is quite the reverse of his royal paramour; his body being thick, short, and clumsy, and very obtuse at each extremity. It is covered also, as to the *head* and *trunk*, with dense hairs.

There are two descriptions of males—one not bigger than the workers, supposed to be produced from a male egg laid in a worker's cell. The common males are much larger, and will counterpoise two workers.

The *head* is depressed and orbicular. The *tongue* is shorter and more slender than that of the female; and the *mandibles*, though nearly of the same shape, are smaller. The *eyes* are very large, meeting at the back part of the head. In the space between them are placed the *antennæ* and *stemmata*. The former consist of fourteen joints, including the *radicle*, the fourth and fifth being very short and not easily distinguished.

The *trunk* is large. The *wings* are longer than the body. The *legs* are short and slender. The *posterior tibiæ* are long, club-shaped, and covered with inconspicuous hairs. The *posterior plantæ* are furnished underneath with thick-set *scopulæ*, which they use to brush their bodies. The *claw-joints* are fulvescent.

The *abdomen* is cordate, very short, being scarcely so long as the head and trunk together, consisting of seven segments, which are fulvous at their apex. The first segment is longer than any of the succeeding ones, and covered above with rather long hairs. The second and third dorsal segments are apparently naked; but under a triple lens, in a certain light, some adpressed hairs may be perceived;—the remaining ones are hairy, the three last being inflexed. The ventral segments are very narrow, hairy, and fulvous.

III. *The Workers* are females, and are of two sorts, the wax-makers and nurses. They may, according to Huber, be divided into fertile and sterile, for some of them, which, in their infancy, are supposed to have partaken of some portion of the royal jelly, lay male eggs.

The *body* of the worker is oblong, and of much smaller dimensions than either of the preceding.

The *head* triangular. The *mandibles* are prominent, so as to terminate the head in an angle, toothless and forcipate. The *tongue* and *maxillæ* are long and incurved: the *labrum* and *antennæ* black.

In the *trunk* the *tegulæ* are black. The *wings* extend only to the apex of the fourth segment of the abdomen. The *legs* are all black, with the *digits* only rather piceous. The posterior *tibiæ* are naked above, exteriorly longitudinally concave, and interiorly longitudinally convex; furnished with lateral and recumbent hairs to form the *corbicula*, and armed at the end with the *pecten*. The upper surface of the *posterior plantæ* resembles that of the *tibiæ;* underneath they are furnished with a *scopula*, or brush of stiff hairs set in rows: at the base they are armed with stiff bristles, and exteriorly with an acute appendage, or *auricle*.

The *abdomen* is a little longer than the head and trunk together; oblong, and rather heart-shaped—a transverse section of it is triangular. It is covered with longish flavo-pallid hairs: the first segment is short with longer hairs; the base of the three intermediate segments is covered, and as it were banded, with pale hairs. The apex of the three intermediate ventral segments is rather fulvescent, and their base is distinguished on each side by a trapeziform *wax-pocket* covered by a thin membrane. The sting, or rather *vagina*, of the *spicula* is straight.

Plate I. fig. 1.—The leaf or book hive, consisting of twelve vertical frames, such as fig. 3, applied parallel to each other, and connected together. The sides *f g*, *f g*, fig. 3, are twelve inches long: the cross spars *g g*, *f f*, nine or ten; the thickness of both the sides and the spars an inch, and the breadth of the sides of the frame an inch and a quarter, which last dimensions should be exactly observed. A piece of comb, *a a*, is introduced to guide the bees in their work, supported on a moveable slider *c*, which may be elevated or depressed by the pegs *e e*. The comb is secured by the pegs *b b*; and in fig. 4, the position of those corresponding on the opposite sides, as well as the whole apparatus, is illustrated.

The twelve frames, all numbered fig. 1, are covered by two shutters, *b b*, at the ends. Between 6 and 7, two frames, *a a*, each with a shutter, are inserted to separate the bees in forming an artificial swarm. The entrance at the bottom of each frame may be opened at pleasure; but the whole, except the first and last, should be kept close. It is explained by fig. 2, showing the hive partly open, how the component frames may be connected by means of hinges, and opened like the leaves of a book.

Fig. 5, a glass box, *a*, for exposing the work of bees while enlarging their combs upwards. Fig. 5, *b*, a box with moveable wooden spars, which can be raised and depressed by a screw at each end, *s s*, for exposing the progress of the cells.

Plate II. All the figures are magnified. Fig. 6, the sexual organs of the drone, as particularly described by Swammerdam and Reamur. Last segment of the abdomen *a*; a membranaceous sac *m*; a lens-formed portion *i l*, communicating by a duct *e r*, with the two vesiculæ seminales *s s*, which are connected with the vasa deferentia *d d*, and the latter with the testes *t t*. A pallet-shaped substance *p*; two appendages *c c*. Fig. 7, a separate view of the portion resembling a lens *i l*, after rupture of the duct. It is composed of two shelly plates, *i e*, *i e*, and two cartilaginous *n n*, together with what is supposed the principal organ *r*.

Fig. 8. Unexpanded sexual organs of the workers; ovaries *a a*; portions of the spinal marrow *b b*; the sting, with its appendages *c*, and poison bag *d*.

Fig. 9. A worker magnified, showing the position of the scales of wax under the segments of the abdomen.

Fig. 10, 11, 12. Comparative configuration of the abdomen of workers, males, and queens.

Plate III. All the figures are magnified. Fig. 13. Segment of the abdomen of a worker, showing the receptacles wherein the scales of wax are produced.

Fig. 14. Scales extracted from the receptacles.

Fig. 15. A reticulation lying under the scales in the segment, fig. 16.

Fig. 17, 18, 19. Views of the teeth or mandibles of bees.

Fig. 20, 21, 22. A limb of the third pair, and its different parts. The leg *c*, where the pollen is collected in a corbiculus or basket. The shelly teeth, *a*, fig. 22, at the junction of the leg with the foot, employed as pincers by bees to extract the scales from under their rings.

Plate IV. fig. 23, 24. A bee extracting a scale of wax, and preparing it for work.

Fig. 25. A cell of the first or foundation row, having a pentagonal orifice.

Fig. 26. Front view of the first and second row of cells, *a*; reverse of the same portion, *b*.

Fig. 27. Foundation and second row of cells magnified to illustrate the different structure of the bottom.

Fig. 28. The bottom of a front cell of the first row, consisting of two trapeziums, *a b*, and a lozenge *c*; also shown in fig. 27, *a b c*.

Fig. 29. The bottom of a reverse cell of the first row, consisting of two trapeziums, *a b*.

Fig. 30, 31. Front and reverse of the bottom of the first and second row of cells. Fig. 30, *f a*, *b g*, the alternate quadrilateral pieces of the former; *g b*, *a f*, those of the latter; *c d e*, the lozenges forming the bottom of the second row.

Fig. 32. Commencement of the first cell.

Fig. 33. Commencement of the first cell on the opposite side of the comb.

Fig. 34. Foundation cell on the front.

Fig. 35. Foundation cells on the reverse, and one of the second row, showing that the former are pentagonal, and the latter hexagonal.

Fig. 36, 37. Blocks of wax in which the cells are excavated.

Plate V. All the figures excepting 45 are magnified.

Fig. 38, 39, 40. Progress of a foundation cell of the first row. The real position of these figures and of some of the others is inverted, though represented upright in the engraving, as bees work downwards.

Fig. 40, *g d a*. The base of the cell or roof of the hive.

Fig. 41. Two foundation cells behind the former, also attached to *h d k*, the roof.

Fig. 42, 43. Front and reverse of the first and second row of cells in progress, according as additions are made to the block of wax.

Fig. 44. Formation of a cell of the second row, showing the curvature of the margin *p r m*, to be converted to the rectilinear margins *p r m* of fig. 42.

Fig. 45. Comb begun under a spar, and continued upwards above it.

Fig. 46, 47, 48, 49, 50. The bottom of transition cells, consisting of four pieces, and their return to three lozenges.

APPENDIX.

I. *Generation of Bees.*—The author has entered on prolix anatomical details, in explanation of the theory of the generation of bees, which many will think uninteresting, and an interruption of the narrative. Therefore, the subject, and several relative experiments, have been abbreviated by the translator.

Swammerdam was unable to discover the external termination of the oviduct of the queen, which the author has ascertained to be in the last ring of the abdomen; also that the eggs are dropped by it into a kind of cavity, from which they are quickly discharged.

He has discovered certain parts among the sexual organs of the male, that are lost in commerce with the queens; particularly fig. 7, Plate II., which he considers as corresponding to the principal organ of other animals.

He obtained a number of females after Schirach's method; and, on the tenth of July, successively released three, which were four or five days old, and still in a virgin state. Two departed several times; their absence was short and fruitless: the third profited better by her freedom. The first and second time her absence was short; but the last continued thirty-five minutes, when she returned in a very different condition, and such as admitted no doubt of what had happened, for she exhibited the organs of the male that had rendered her a mother. They were disengaged by herself; she was allowed to enter her hive, and its entrance adapted so that she could not leave it unnoticed. No eggs were found in it on the seventeenth; the queen was as slender as on the first day, therefore she had not been fructified. Being again set at liberty, she departed twice, and returned with evidence of a second union. The eggs which she laid after this event showed that it had been more successful; but a repetition is rarely requisite. It was remarked on only two occasions.

On the eighteenth, a virgin queen twenty-seven days old was released; she departed twice. Her second absence lasted twenty-eight minutes, when she returned with the proofs of her union. On the 19th another queen, four days old, was set at liberty; she also departed twice. Her first absence was short; the second continued thirty minutes, when she returned with the evidence of fecundation.

In a subsequent experiment, two virgin queens were liberated on the twentieth. The first had been abroad on the preceding days, but the scarcity of males had prevented her impregnation. She now returned with the wonted organs, of which she divested herself with her feet. The second queen departed twice. Her first absence was short as usual; the next lasted half an hour, when she returned impregnated.

It appeared to the author that additional evidence of the preceding facts might be obtained, by seizing the male on its return which had rendered a queen a mother; and this could be expected only if sudden death did not follow. His assistant, believing that he could easily distinguish those dying unmutilated by their commerce, engaged in examining the whole found dead at the hives during the season of swarming. After a painful research, some were actually discovered dead, and clearly in a mutilated state. None of their usual organs remained, or only some decayed portions of them. It was thence established that the males perish, but that mutilation is not followed by immediate death, as they have time to return to the site of the hive.

II. *Secretion of Wax.*—The author had committed the dissection of bees to Miss Jurine, a young lady distinguished by her talents and her taste for natural history, for the purpose of investigating the organs destined for secretion of wax.

On carefully removing the four wax segments of a worker, this skilful anatomist found an adipose membrane interspersed with tracheæ, and perfectly resembling that which had been recognised by Swammerdam, below the upper segments of the abdomen. As it exists beneath all the segments, while wax appears only in the lower ones, it is not to be presumed the secretory organ; and from examining the abdomen of the violet bee [sylocopa violacea,] and of two species of wasps, she was satisfied of the fact.

A whitish membrane, covering the internal surface of that portion of the wax segments provided with the receptacles, was afterwards discovered. It could be easily removed by maceration; and, presented to the microscope, appeared to consist of a beautiful net, of very small hexagonal meshes, full of a liquid as thick as syrup. It was also found under the same segments of the abdomen of humble bees, but occupying all their inferior half. This membrane is most perceptible when bees are building their combs; at other times it is indistinct, but then it is so full of the whitish matter, that it might be taken for scales of wax.

Detaching the reticulation from the ring to ascertain whether it contained real wax, or only preparations for it, and taking the scales of wax themselves,

M. Jurine put each into a separate vessel, and poured boiling water over them. The latter melted, but no waxy particle escaped from the former.

She likewise endeavoured to discover the vessels communicating with the reticulation, and how the wax transuded from the abdomen. However, on dissecting a number of bees, nothing except minute tracheæ, immediately communicating with the reticulation, were discernible. Following another plan, she fed bees for some days on honey, coloured with lac; yet this substance did not reach farther than the digestive organs, and injections of mercury were attempted as unsuccessfully.

M. Jurine conjectured that wax might be formed by a transudation through the stomach, which is very much gorged while bees work in that substance. She found, by gently pressing the stomach of several wax workers, half the liquid it contained escaped into the abdominal cavity. Its taste was saccharine; and the same bees having been afterwards exposed to moderate heat, it assumed the consistence of dried syrup, Bees having several methods of obtaining such a pressure, she asks, may it not be attended with similar effects; and may not the liquid transuding the stomach, reach the hexagonal reticulation, where it receives a preparation suitable for its conversion to wax?

Unable to discern any channels of communication, M. Jurine stretched out the abdomen of several workers having wax scales, to remove them with more facility. Then pressing the wax segments with the head of a needle, she saw their receptacles insensibly moistened with a liquid, of the consistence of syrup, observed nowhere besides. It acquired still greater consistence, but no waxy appearance, when the bees in this condition were exposed to heat.

The same experiment having been repeated on bees that had been dead several days, and were somewhat dried, the wax scales broke into fragments on attempts to remove them. By the simple expedient of pressing the wax segments several times, the scales could be obtained entire; nor was this to be ascribed to any thing but the oozing of the matter like syrup into their receptacles, as had been remarked in the preceding experiment.

The reticulated membrane, which occupies none excepting the wax receptacles in workers, is replaced in queens by one extending over two-thirds of the anterior part of each segment. Its fine and delicate texture is ascertained by means of the microscope. Removing this membrane, M. Jurine remarked, that the organization of the scale exhibited a hexagonal tissue, much more decided in the half of the segment corresponding to the wax receptacles of the worker, than to the posterior half. Being thence led to narrower inspection of the shell of the wax segment of workers, she found it perfectly smooth in the part where the receptacles are situated, and resembling that of the queen throughout the rest of the segment.

Males are altogether destitute of the adipose membrane and the hexagonal reticulation; and muscular fibres with tracheæ, disposed as in the worker, appears in their place. The organization of the shell of the rings displays the same hexagonal tissue as that of the queens.

III. *Origin of Wax*.—The author gives an extract from a Memoir by the late Dr John Hunter, wherein preceding opinions on the formation of wax are questioned.

He observes, that the materials forming the comb are in a different state as a composition, from what they appear in any vegetable where they are found. The pellets brought home on the legs of bees, which had been previously supposed the source of wax, are only the farina of flowers. Endeavouring to ascertain whether they contained a quantity of oil sufficient to account for the quantity of wax formed from them, and to learn whether they contained oil, he held one near a candle; it burnt, but without the smell of wax, and emitted the same smell as farina did in burning.

Dr Hunter remarked that the pellets are of different colours on different bees, but always of the same colour on both legs of the same bee; whereas the shade of new made comb is uniform. The farina is collected with greater avidity for old hives, where the comb is complete, than for those where it is only begun, which would hardly be the case were it the elements of wax. Two days usually elapse before the bees of a new colony bring home pellets on their legs, and then the number of gatherers increase, because some cells are now formed to receive it, and some eggs are laid whose young will soon require it for food.

When the weather was so cold or so wet in June as to prevent a young swarm from going abroad, as much comb was constructed, nevertheless, as had been made in an equal time when the weather was favourable.

Wax, in Dr Hunter's opinion, is formed by the bees themselves; it may be called an external secretion of oil, and originates between each plate of the under side of the belly. On first observing it, in examining a worker, he was embarrassed to explain its appearance, and doubted whether new plates were forming, or whether bees cast the old ones like the shell of lobsters and other animals. He found it nowhere but between the rings below. Most of the bees climbing the panes of a glass hive exhibited this substance; and it gave the lower or posterior edge of the segment the resemblance of being double, or there seemed to be double scales; but it was loose, and did not adhere.

Having proved that the pellets of the limbs were farina, apparently destined for the larvæ and not for wax, and having seen nothing else which could be supposed the source of it, he conceived that the scales might be wax itself. Several taken on the point of a needle, and held to a candle, melted into a globule, which confirmed his opinion of the fact; and he was the more satisfied of the truth, from not finding the scales, unless during the season that bees construct their combs. However, he was unable to seize the moment of the bees detaching the scales from themselves, though he conjectured they might be taken up by others, if shaken out from between the rings. It is with these scales that the bees construct their comb, but perhaps when the secretion is not in abundance, they mix a little farina with it.

Dr Hunter, besides, makes several interesting observations on the structure of combs.—*Philosophical Transactions*, for 1792, p. 143.

IV. *Farina, or Pollen.*—Farina, or Pollen, is the fertilizing dust of flowers, and forms a very important ingredient in the nourishment of the young bees. Before the discovery of the true origin of wax, it was supposed to constitute the rude material of that substance, being taken into the stomach, and converted by some peculiar action of that organ into real wax; and hence, among French naturalists, it had obtained the name of *cire brute*, or crude wax. It consists of an infinite number of small globules, which, in exploding in consequence of the application of moisture, shed a subtle essence over the pistils of the flower, and thus effect the fecundation of the plant. The bees eagerly set about collecting this nutritious substance as soon as the season affords it, and continue to do so throughout the summer, not only for immediate use, but also for storing up against the season when it is not to be obtained abroad. They may be observed upon the anthers of flowers, gathering this substance with unceasing activity, and forming it into little lenticular-shaped pellets, which they place in the baskets in their third pair of legs. They often roll their bodies in the flower-cup, and then brush off the pollen adhering to them; and they are sometimes seen tearing in pieces the capsules containing it, in order to get at their object. The colour varies according to the hue of the flower from which it is collected. In spring it is generally of a bright yellow or orange, as these are the prevailing colours of the early flowers, such as crocuses, snow-drops, turnips, furze, &c. The bee, in each excursion in search of this substance, visits only one species of flower. This is proved by the fact that the little balls with which they are loaded are uniformly of one unmixed colour—a wise provision of Nature, for thereby is the insect instinctively led to collect, at the same moment, those particles only of farina, which, being homogeneous, will form the closest cohesion, and is further prevented from contributing to the multiplication of hybrid plants.

The collection of pollen by the bees is made in greatest quantity in the earlier part of the day, before the heat of the sun has dried up the moisture which renders it more easily packed into the little masses which adhere to their legs. After they are fully loaded, they return to their hive, and deposit their burden in cells in which there is neither honey nor brood. The mode in which the bee unloads itself has been already noticed. Planting her middle and hind legs firmly on the edges of the cell, she sweeps with her fore legs the pellets from their baskets, and thus drops them into the cell. Another worker instantly inserts her head into this cell, and keeps it there for a minute or two, evidently kneading the farina, and probably mixing with it a portion of honey disgorged from the honey-bag, as it presents a moist appearance on her leaving it. Farina is probably mixed with wax in constructing the combs when the latter substance is scarce, especially in building the royal cells, the *outer* surface of which appears to be nearly altogether farina, and only the *inner* surface of wax highly polished. But the principal use of this substance, after undergoing, perhaps, a peculiar elaboration, is to nourish the brood. This fact was proved by an interesting experiment of Huber—which see *Naturalists Library, vol. VI.*

V. *Nature and Production of Honey.*—Honey is well known to be a vegetable product, secreted in the nectaries at the base of the corollæ of flowers. It has been supposed by some writers to be the elemental principle of all vegetables, without exception, and indispensable to their existence; although there is perhaps, no sufficient evidence of the saccharine matter of plants being in all cases convertible into honey. As one of its secondary uses, it seems destined by Nature for the food of bees; and these industrious collectors fail not to appropriate the rich liquid. Sweeping the hollow of the honey-cup with their little probosces, the skilful little chemists eagerly imbibe the saccharine juices as they exude from the nectarium, receive them into the globular honey-bag, which forms their anterior stomach, and hurrying homewards with their precious load, disgorge it into the cells prepared for its reception. The quantity which each bee deposits at one time is very small, the honey-bag, when full, not exceeding the size of a pea; but the aggregate quantity collected by the whole population is prodigious. When the cell is full, it is carefully sealed with a waxen cover, and reserved for use in winter and spring, particularly in the latter season; for more honey is consumed in the months of March and April, when breeding goes on actively, than during the four preceding months. At the same time, many cells are left open, and half-filled only, for daily consumption. It has been a subject of discussion among naturalists, whether the honey, after being extracted from the flowers, undergoes any change in the stomach of the insect before being deposited in the cell. Feburier is of opinion that it is subjected to the digestive process. The celebrated John Hunter thought it remained pure, and in no respect whatever altered, however long it had been retained in the stomach of the bee; and he is followed in this conclusion by his countryman, Bonner. Kirby and Spence, entomologists of no mean fame, have adopted the opposite opinion; but it does not appear that they had been led to this conclusion by the result of any experiment instituted for the purpose of deciding the matter. Reaumur, however, tells us, that, from his experiments, he was satisfied that a process of elaboration does take place in the food with which he had supplied his bees, and that the sugar with which he fed them had precisely the taste and flavour of honey. Our experience, if we may venture to differ in the matter from men so deservedly celebrated for attainments in natural science, leads us, with Hunter and Bonner, to a different conclusion. We have repeatedly tasted the syrup of sugar, which we had seen the bees taking from the feeding-trough, and depositing in the cells, and could never discover the slightest difference in any respect, at least so far as taste and flavour are concerned. Perhaps the liquid was clearer—we sometimes imagined it was—if so, this constituted the only difference.

The secretion of honey depends greatly on the state of the atmosphere. During the prevalence of dry easterly winds, the fields present to the bees nothing but barrenness; their out-door labours are suspended, and but for the already hoarded stores, the brood would be in imminent danger of starvation. But when the weather is moist and sultry, and the air charged with electricity, the circulation of this vegetable fluid is considerably accelerated, and the bees know well how to avail themselves of so favourable a juncture for collecting their treasure. Huber remarks, that the collection is never more abundant, nor their operations in wax more active, than when the wind is from the south, the air moist and warm, and a storm approaching. Heat too long protracted, however, and its concomitant drought—chill rains and a north wind, entirely suspend the elaboration of honey in vegetables, and consequently the operations of the bees. The quality of the saccharine fluid is influenced by various causes. Something depends on the particular period of the season in which it is collected. In Scotland, the best honey is gathered in the months of June and July, when the *white clover* is in bloom: and what is stored in spring, or rather in April and May, is purer and better flavoured than what is obtained in autumn, unless the bees have been during the latter season within reach of *heath*, the honey from which is of a rich wild flavour, but of a darker colour. The quality of honey is, of course, much influenced by the nature of the plants most frequented by the bees. The famed honey of Hymettus derives its excellence, it is said, from the *wild thyme* growing so luxuriantly on the celebrated mountain from which it derives its name; that of Narbonne, from the *wild rosemary*. The white Dutch clover and the heath have been already noticed as furnishing honey of a superior kind, and there is a district in Galloway, North Britain, where perhaps the best honey in the kingdom is produced, owing, it is supposed, to the great abundance of *wild thyme* with which the country abounds.

We have seen it remarked in bee-publications, that the finest honey is got from young swarms; the fact is so, generally speaking, but not, as we might naturally be led to infer from the assertion, because it is the produce of young bees or of fresh swarms, but because bees swarm only at the height of the honey-season, when the flowers are in their richest fragrance, and because the combs are then new, and have not as yet served as receptacles for the brood. The above remarks apply to the quality of the honey in the state in which it is secreted in the flowers; its after-treatment does not improve it. The heat and vapour of the hive are injurious to it; in very severe seasons it is sometimes candied; and in the honey-harvest, when it is being separated from the wax, its purity may easily be injured by imperfect management.—*Naturalist's Library, vol. VI.*

VI. *Varieties and Species of Honey-Bees.*—The cultivation of the common honey-bee, in the warmer countries of Europe, being an object of the utmost consequence to the farmer, every means that ingenuity could devise to improve the breed and management of these profitable creatures have been adopted, and with success. They distinguished three kinds or varieties of the common bee (*apis mellifica*). The first is large, and of a deep brown colour; the second is smaller and blackish; those of the third sort called "the little Flemings," or "little Hollanders," are much smaller than either, and of a fine glossy yellow colour. It is the latter that is very generally cultivated on the continent at this time. Apis mellifica is an European insect. Mr Hunter supposes it an inhabitant of Asia and Africa also; its appearance in America may be accounted for on the presumption that it was originally introduced there from Europe, and in the course of time has become completely habituated to that climate. It is said to have been originally peculiar to the continent of Europe, but this will admit of doubt. In those parts of Asia and Africa nearest to the south of Europe, they cultivate the same kind as ourselves. There are some other species of bees domesticated like the common bee with us, in different parts of the world; and others again, whose wax and honey are sought after by the natives, who do not care to take the charge and trouble of domesticating them. In Cayenne and Surinam, the species called by Olivier *amalthea*, is an abundant and most profitable creature, This little bee is of a black colour, with white wings and long posterior feet. They build their nest in the shape of a bag-pipe, upon the tops of the highest trees. The honey is very sweet and agreeable, and thin, and of a reddish colour. From the latter the Indians extract a spirituous liquor, of which they are passionately fond; of the wax they make candles. This is supposed to be the small black innoxious wood-bee of Barrère, which is called *ouano* in Cayenne. M. Latreille mentions this species, and also another, which he calls "Pabeille sociale" *(apis socialis)*, among his "apiares domestiques," an insect rather smaller than the common honey-bee *(mellifica)*, that is found in India. Specimens of it, he tells us, were received at the museum of natural history in Paris, among a collection of other insects from Bengal. If we are not mistaken in the species, the same kind was likewise introduced into the cabinets of the curious in this country some time ago by Mr Fitchtell, who found it to be very commonly cultivated by the inhabitants in the vicinity of Bengal.—*Rees' Encyclop., art. Bee.*

VII. *Most common Enemies of Bees.*—In the domestic state bees have many enemies, but in a state of nature these are by far more numerous. As it is with the former we have more especially to do, we shall shortly mention those known to be most destructive to the hive, with the remedies which have been proposed by celebrated bee-masters. Huber relates his discovery of the ravages committed by the *sphinx atropos* (page 221), this we shall not repeat, but limit ourselves to the list given in *Ree's Encyclopædia*, article Bee:—While in the apiary, wasps and hornets are among the most formidable of those enemies; they will often contrive to enter the hive, and build their nests in it, and harass the bees without mercy, till they leave their habitation, unless proper care be taken to prevent such encroachments. The fox is a dangerous enemy in the winter, as he is able to make a passage into the hive, and devour the honey. Rats are equally injurious; the house and field-mice should also be guarded against, by diminishing the entrance into the hive, as the cold comes on, when the bees become less able to defend themselves. The hives may be placed in such a manner that it will be impossible for the mice to reach them. Birds are bitter enemies to the bees; the sparrow, house-lark, and swallows in particular. Toads and frogs will place themselves at the entrance of the hive, and devour many. Spiders will expand their snares near the hive, and entrap numbers. The species *aranea calcina* lies in ambush for the bees in the corolla of flowers, and fastens upon them when they come to sip the nectareous fluids. Ants of almost every kind penetrate into the hive, attack the young brood, and plunder the combs of the honey. The stink of certain species of ants is so offensive to bees, that they will quit their hives to avoid it, or if they remain, become sickly. Some larvæ, or caterpillars, are likewise exceedingly injurious to the bees, the honey, the comb, and hive. *Phalæna mellomella*, or honey moth, too frequently secures its residence in the hive, and deposits its eggs; which hatching produces a larva of a pale flesh colour, that subsists entirely on the honey. The eggs of another phalæna, the wax moth, *P. cerella*, give birth to far more destructive larvæ than the former: for these no sooner burst from the eggs, than their operations commence; they attack the comb, which they perforate in a variety of intricate passages, burrowing and feeding as they proceed, till they reach the bottom of the cells in which the bees are lodged; here they remain in security, and not uncommonly compel the colony of bees to leave their residence. The old combs are those that are generally infested by this creature. A third sort of moth, *phalæna sociella*, breeds likewise in the honey-combs of some bees.

Hives of bees that have swarmed more than once, and such also as contain but little honey, are most exposed to the depredations of these insects; for the half-exhausted combs serve to shelter them, and the scanty store of honey or wax supplies them at least with food to the detriment of the colony. Bees are subject also to a peculiar species of *pediculus*, called the bee-louse. Hives of bees that have swarmed more than once, and such as contain but little honey, are most exposed to those troublesome vermin. The hives in this case should be cleaned at the farthest once every week, and the stools on which they stand every morning, for the latter are likely to harbour the larvæ and moths, or other insects, as well as the hive. But these obnoxious creatures cannot be entirely extirpated without taking away the infected hive, removing the bees, and cleansing it, before it is restored to the former station. The lice of bees are of a slender shape, or filiform, and of a ferruginous colour, and may be destroyed by strewing tobacco over the bees.

VIII. *Maladies of Bees.*—In the spring the bees are subject to a kind of dysentery, which proves often fatal. The matter which they void at this time, when so affected, instead of being of a reddish yellow colour, is of a muddy black, and has an intolerable smell. Columella supposed this annual distemper to be occasioned by the bees extracting too freely the juices from the blossoms of the spurge and elm trees, or, as others believe, from the lime tree. There are writers who, dissenting from this opinion, attribute it to the quantity of new honey, of which they are known to eat to excess at that season of the year. Again, others imagine that it is caused only by their long stay in the hive during the winter, when they are constrained to feed on the coarse wax, if their honey fails to afford them a sufficient quantity of food. Madame Vicat, in the "Memoires, &c." of the Berne Society for 1764, ascribes this distemper to the honey, which the cold has candied in the hive during winter. The true cause of this distemper seems to be unknown; but it is certainly contagious and very destructive. A good remedy for it was long unknown. Aristomachus recommends the removal of the vitiated combs. For the recovery of the bees affected with this distemper, a new remedy has been adopted upon the continent: they prepare a syrup composed with an equal quantity of good wine and sugar, which is administered to the bees in every hive, either by pouring it into the cells, or placing it within the hive in a saucer, or any other shallow vessel; this has been found an excellent restorative.

About the end of the spring, another disorder sometimes makes its appearance, which Du Carne de Blangy calls a "vertige," or vertigo. This is supposed to be occasioned by the venomous properties of certain plants on which they feed. The symptoms are manifested by a dizzy manner of flight, by their involuntary startings, falls, and other gestures, in attempting to perform their usual operations, or in approaching the hive, and by the lassitude that succeeds these symptoms. This distemper has been hitherto found incurable.

Bees are liable to a third distemper, the symptoms of which are a swelling at the extremity of the antennæ, which becomes also much inflamed, and of a yellow colour; the head assuming shortly after the same tint, the bees lose their vivacity, and languish till they die, unless a proper remedy be applied. In France, they give them Spanish wine for this disorder.

There is still another distemper which sometimes makes its appearance among bees, for which the continental agriculturalists administer Spanish wine, as in the former cases. This is a kind of pestilence by which many bees are cut off. It happens when the queen bee has placed the eggs carelessly in the comb, so that the larvæ perish in the cells, or that they are killed by the cold, or bad management in nourishing and feeding them; when numbers die, and infect the rest. The only attention requisite in this case is to take away the infected combs, scent the hive with the perfume of aromatic plants, and give them the wine to sip, as above mentioned, in order to strengthen and restore them from their sickness.—*Rees' Encyclop.*

IX. *Effects of Cold.*—Cold, generally speaking, is prejudicial to bees. When tempted by a bright sun after a fall of snow, a few have left their comfortable dwelling, they are quickly chilled, and in a very short period are past recovery. But with ordinary precautions, in stopping crevices, and providing a sufficient external covering, a well-peopled and well-provided hive runs no hazard from even the severest winter. Consistently with that wisdom that shines forth in every part of creation, insects that feed upon leaves, flowers, and green succulent plants are generally in a torpid state during the winter, when they cannot procure for themselves subsistence abroad. Bees are in this state, and eat little, while cold weather lasts; but they are by no means in so complete a state of torpidity as to eat none at all. On a mild day in winter, when the sun shines and the wind is low, we often observe them eagerly taking advantage of this favourable temperature, and coming abroad in hundreds to enjoy themselves in the open air. If we open a leaf-hive in the very depth of the cold season, we shall find them closely clustered together, but in near contact with the provision-cells; and the whole mass moving without separating, and by this means doubtless contributing to preserve the general warmth.

It has been made a question among bee-writers, whether a mild or a severe winter be most favourable to the health and well-being of these insects? Bonner and others are advocates for mild winters; while

White, Bevan, &c., maintain that severe winters are most salutary. We are of opinion that the question admits not of a *general* determination, but that *special* regard must be had to the state of the hives—a circumstance which has been too little taken into account in the discussion. In a well-found hive, it is of very little importance to the inhabitants of what nature the winter may be. If it is severe, they have enough of internal heat to preserve them from the severity of the external atmosphere. Huber found that when the thermometer in the open air stood several degrees below the freezing point, it rose, when plunged into a populous hive, to 86 and 88 degrees. Swammerdam observes that the heat of a hive is such, even in the middle of winter, that the honey never crystallizes, unless the hive be very weakly peopled. Reaumur found brood of all ages in the month of January; and the same thing was experienced by Huber, when the thermometer within the hive stood at 93 deg. If, on the other hand, the winter be mild, the bees consume food partially, and frequently go abroad into the open air; and by thus voiding their excrement preserve themselves in health. But the case is far otherwise with a hive thin in population, and scantily provisioned. In severe weather, their numbers are too few to keep up the vital warmth, and they are in imminent danger of perishing, should the cold continue for a lengthened period. Should the winter be mild, they consume their stores; and on the arrival of spring, if they still survive, they run the hazard of perishing of hunger. We are decidedly of opinion, therefore, that the temperature of the winter has much less influence on the prosperity of the apiary, than is generally imagined; and that the bees coming safely through that inclement season, depends in almost every case on the *abundance of population and of food*.—*Nat. Lib.*

XI.—*Situation of the Apiary.*—In the practical management of bees, the formation and due arrangement of the apiary is of some importance. The prime requisites are *shelter* from the extremes of heat and cold, and *quiet*. Facing southwards, the hives should be carefully screened from the north and north-east. A group of young trees, or a close-growing hedge will answer the purpose well; or advantage may be taken of a range of buildings, or a garden wall. In availing ourselves, however, of the shelter of buildings, care must be taken to keep the hives at such a distance as to be clear of the rain-drops, and from the eddying winds caused by such a locality. A distance of not less than eight or ten feet should intervene between them and the screen; and of this space the half-breadth next the hives should be laid with fine gravel, to absorb the moisture, and keep it free from weeds, grass, straws, &c. The space of ground between and in front of the hives, to the extent of at least three feet, should be covered in the same manner.

Quiet is essentially necessary to their doing well. Bees do not thrive in the near neighbourhood of incessant noise. The apiary, therefore, should be at a distance from smithies, mills, steam-engines, &c., and also from such manufactories as emit noisome smells. The hives should be elevated about fifteen inches from the ground, on a *single post* or pedestal, in preference to three or four, which is the usual number. Vermin are thus prevented by the projecting edge of the floor-board from climbing over and reaching the entrance. It may be laid down as a good rule to have the hives placed as far from one another as the extent of the apiary will admit. When standing at intervals of only two or three feet, the bees are very apt to quarrel amongst themselves. They sometimes mistake their own proper domiciles when too much crowded together, especially when hurrying homewards in the working season, or hastening to escape a shower, and the mistake is attended with fatal consequences. In feeding a weak hive, a close neighbourhood is particularly dangerous; the smell of the syrup is quickly diffused over the whole colony, and pillage generally ensues. In swarming, too, when the newly departed emigrants are discouraged by a sudden blast or change of atmosphere, and the queen hastens to return to her old abode, her ignorance of the locality, having, if a young queen, never been abroad before, renders her very apt to mistake and enter a hive where she is by no means welcome, and, the swarm following her, a bloody conflict takes place. All these inconveniences point out the propriety of a large interval between the hives, and this arrangement is especially called for when, as in very extensive apiaries, the hives are placed in double rows. We do not approve of double rows; they occasion great confusion often in the swarming season. If the number of hives be too large for a single row, let there be a second group formed in another quarter of the grounds. This detached apiary will be found useful in such operations as require the temporary removal of stock-hives from their original stations.

Water is essential to the operations of these insects during spring and summer: a shallow pebbly stream in the vicinity will, therefore, be most advantageous, where they can drink without danger of drowning. Its absence should be supplied by artificial means; and a shallow vessel of water placed in a secluded and quiet quarter of the apiary, having a few smooth round stones thrown into it, of a size to project above the surface, and afford footing to the drinkers, will answer the end. The neighbourhood of large sheets of water, however, or of broad rivers, is injurious; the little foragers, in crossing during high winds or dashing rains, perish by hundreds in a single day.

Covered apiaries, or bee-houses, are common in England, and are sometimes, though rarely, met with in Scotland; they have their advantages, but are not without serious drawbacks. They afford shelter from the extremes of heat and cold, and, when properly constructed, are also a complete protection from thieves. But when the number of hives is great, the expense of such structures is so considerable as to preclude entirely their being brought into common use. Besides, their confined limits render it necessary to place the hives quite close to one another—an arrangement which we have already noticed as a great evil. A good thick coat of oat or rye-straw, if the hives be of that material; or, if of timber, a well-seasoned and painted surtout of fir-plank, three-fourths of an inch in thickness, resting on the floor-board, and having a vacant space of an inch between it and the hive, will be quite sufficient security against the extremes of heat and cold.

Of course it is highly in favour of the bees when the apiary is situated in a country abounding with such natural productions as the industrious insect can turn to account. Large heaths, sheltered with woods, are extremely productive of honey, as the wild thyme and other flowering plants with which they abound, are not cut down by the scythe; and the heath itself remains in bloom till late in the season. The plane-tree, the whole willow tribe, the furze or whin, the broom, especially the Spanish kind, furnish a rich store both of honey and farina. The bees do not feed indiscriminately on every species of flowers; several of the most splendid and odoriferous are wholly neglected by them, while they select others, the flowers of which are extremely small, and not apparently possessed of any very valuable qualities. Moreover, they give a decided preference to those spots where a *great quantity* of their favourite flowers grow together. On the continent, fields of buckwheat afford a copious supply, though the honey extracted from it is of a coarser kind; and in our own country, the white clover (Trifolium repens), will, in fine weather, be found thronged with them, while scattered plants that afford more honey are neglected. When a variety of bee-flowers flourish in the same field, it is said they will first collect from those which furnish the best honey; if, for example, several species of thyme grow together, they prefer the lemon thyme, which is of a richer fragrance.

The bee-master will do well to supply his favourites with such flowers, &c. as are not found growing spontaneously in his neighbourhood. In addition to the gooseberry, currant, and raspberry bushes, and the several orchard trees, the flower-borders in his garden should be well stocked with snow-drops, crocuses, wall-flower, and, above all, with mignonette, which affords honey of the richest flavour, and which continues flowering till the near approach of winter. The rich melliferous blossoms of the *Buddlea globosa*, too, the bees are very fond of; and some of the Cacalia tribe afford an ample store. "The *Cacalia suaveolens*," says Darwin, "produces so much honey, that on some days it may be smelt at a great distance from the plant. I remember once counting on one of these plants, besides bees of various kinds without number, above 200 painted butterflies, which gave it the beautiful appearance of being covered with additional flowers."* Besides these, the plants of Borage (Borago), and viper's Bugloss (Echium vulgare), yield a very considerable quantity of the rich liquid. The former is eagerly resorted to by the bees; it is an annual, and blossoms during the whole season till destroyed by frost. In cold and showery weather, the bees feed on it in preference to every other plant,

* Economy of Vegetation, Canto IV.

owing to its flowers being pendulous. The Bugloss appears as a troublesome weed among corn, and grows on dry soils in great profusion; it is a biennial plant. Turnips, particularly the early garden kind, should be sown and allowed to remain in their beds during the winter; and they will in consequence, by their early flowering, afford a seasonable supply of farina, and also a small portion of honey early in spring. The whole cabbage-tribe also may be made to contribute their share; and mustard-seed, when sown in successive crops, will continue blossoming for many weeks.—*Ibid.*

XII.—*On the Management of Bees in Spring.*—About the first or second week of February, unless when the season is stormy, the bees will be observed venturing cautiously to the mouth of the hive; and if the sun shines out about mid-day, the little eager foragers will be seen spreading their wings joyfully, launching forth into the air, though with a low timid flight, and roaming from bush to bush in search of some plant that may yield a modicum of farina—for the Queen has already begun to lay the eggs of workers; and although there is always a certain quantity of this kind of food in the hive (the product of the preceding year's gathering), for the coming brood, the provident insects are aware that an additional supply will be required, and rouse themselves accordingly from the winter's inactivity. The collection of farina, however, is, at this early period, very scanty. The few bees that are seen, during the month of February, entering their domiciles with their yellow loads, derive them almost solely from the snowdrop, the crocus, and the furze-blossom. At this early period, therefore, the owner cannot help them, however anxious to do so, as far as farina is concerned. In other respects, however, equally important, he has it in his power to minister essentially to their welfare, namely, by supplying them plentifully with honey or syrup of sugar. In the article of honey, none of the insect families of a judicious bee-master will be deficient; he has, it is to be presumed, kept none as stock-hives which did not possess stores sufficient, and more than sufficient, to carry his bees through not only the winter months, but those of spring also. But even to the well-provisioned, a little additional supply will be welcome, and prove advantageous, infusing fresh spirits into the hard-working labourers, encouraging the laying of the Queen, and consequently contributing greatly to the rapid increase of the population, and to the production of early swarms. We need not fear being over-liberal; the bees are excellent economists, and will carefully husband what we entrust to them.

The first care of the cultivator, after the appearance of his bees in spring, is to inspect his hives. Lifting them gently from the stool, he will sweep away all the dead bees, eggs of moths, scrapings of wax, mouldiness, or other offensive matters that have accumulated during the winter, and clean and dry the floor-board effectually. The lower part of the combs, where the population is scanty, is sometimes found to be mouldy; it will save the workers much trouble, and contribute to their health, to cut those parts away. Let the cover, if of straw, be next taken off; mice are often found lodging between it and the hive, and, secure from observation, work their way down into the interior. The cover should be renewed, and carefully fastened close to the hive by one or two wooden hoops. As the consumption of food in spring is very great, in consequence of the prodigious quantity of brood reared—the queen laying at the rate of 100 or 200 eggs daily—the cultivator must see that there is an abundant supply, and commence feeding, if there appears anything like a deficiency.* No branch of bee-management requires more attention than the feeding operation, and very many hives, we fear, are irretrievably injured by the injudicious manner in which supplies of food are administered. Giving them in a cold state, or in a state of fermentation, or at improper periods, costs every year the lives of thousands of bees. No food should be given in spring till the bees shew, by their coming abroad, that it may be offered them with perfect safety. A simple mode of feeding is by means of a small drawer, having a float pierced with holes, inserted in the thickness of the floor-board, at the back of the hive. Liquid honey, or syrup of sugar, a little warm, may be poured into this drawer in the evening, after the bees have retired in-doors from the labours of the day. It is taken up immediately, and the smell is completely gone before the morning.

It is of very material importance in feeding, to guard against the admission of stranger bees to the feeding vessel. This may be effected by shutting up the hive completely after the feeding-drawer, above described, has been inserted, allowing only the admission of air. One circumstance, however, may render this precaution abortive; some of the liquid may be, and very often is, accidentally spilt in pushing the trough inwards, the consequence of which is, that the smell of the syrup, when the hive is opened, will attract strangers, and eventually lead to plunder. It is a good method, therefore, to administer the food, when it is given at the external entrance, in a covered vessel, having its opening at one side placed close to that of the hive, so that the bees proceed directly to

* The food given to bees in autumn may be either honey or sugar; but in spring it should always be honey, as sugar does not form so good an ingredient of the jelly which nourishes the young brood.

the trough, without having any communication with the open air, and, consequently, without affording an opportunity of admittance to strangers.

As the season advances, the spring flowers appear in greater abundance, the gooseberry and currant bushes furnish both honey and farina, the seeding turnips and early sown mustard present a very considerable supply ; the furze, also, is in full bloom, and the bees become less dependent on artificial feeding. But, unless the weather be remarkably mild, and the stocks of more than ordinary richness, the adventitious supplies ought not to be withdrawn till the beginning of May. During March and April, the activity and bustle of the hive are greatly augmented, and the industrious foragers may be seen in a genial morning hurrying with their loads into the hive in crowds, and jostling and driving one another about with most unceremonious haste. In a strong hive, from 50 to 70 bees, as already stated, may be observed entering in a minute ; and, when about to purchase a hive, we cannot have recourse to a more decisive testimony of its strength than the numbers that enter loaded with farina in a given period of time. It is, in fact, during this season, about the beginning or middle of April, that such purchases can be made with less risk than during any other part of the year. The winter is past, and the more trying season of early spring, especially the latter half of February and the whole of March, during which periods more bees die than at any other. Their consumption of honey is then so great, from the circumstance of the Queen having begun her laying, and the rapidly increasing quantity of brood, that none but well provisioned hives can support the expenditure. In April, however, the industrious insect begins to get something out of doors; besides the gooseberries and currants, the seeding turnips and furze, the willows are putting forth their catkins, and the buds of the plane and horsechestnut are swelling, all of which contribute to relieve the winter magazines and render it quite safe for a buyer to set about forming his apiary. Let him, therefore, choose a fine morning, when the bees are busily engaged in carrying in farina, and observe attentively, and in their turn, all the hives from which he is to select his purchase, counting the number of each that enter within a minute's space. He will fix, of course, on those that exhibit the greatest number.

The cultivator will sometimes at this season discover, to his mortification, that one or more of his hives has been totally deserted by the inhabitants. If there is no want of honey in the combs, and no appearance of mice or other vermin having obtained access to it, the probable cause of this desertion is the death of the queen during the winter, from age or from accident. In such circumstances, the whole population will gradually leave their habitation; and while many wander about in the cold, and ultimately perish, others may be seen dispersing themselves among the other hives in the apiary. The owner should in this case shut up the hive, carry it into a dry place, and reserve it for a late swarm, to which it will be a valuable acquisition.—*Ibid.*

XIII.—*Methods of Taking the Honey.*—There are three modes of taking the honey, each of which has its advocates; namely *Partial Deprivation*, applicable to storied and leaf hives; *Suffocation*,—and *Driving*, that is forcing the bees to quit their magazines, and uniting the expelled inhabitants to the stock-hives. *Partial Deprivation* consists in appropriating early in the season a portion of the stores. In preparing prospectively for thus sharing in the products of the hive, the cultivator who pursues the storifying system, immediately after the swarming season is over, adds another story or box to the two of which his hive consists, placing it undermost, or as it is called by some bee-masters, *Nadir-ing*. The brood-combs contained in the uppermost story, will, as the young bees are hatched, be quickly filled with honey, and may be removed about the beginning of August. The top cover is then replaced on the next story in position, which was originally the lower, and is now the upper. In ordinary seasons, the bees will have ample time to lay in sufficient food for winter and spring use, after the abstraction of this portion of their stores. As the combs of the upper box are frequently found adhering by their lower extremities to the bars of the next, it will be necessary, before removal, to separate them by means of a very thin long-bladed knife or a fine wire (a pianoforté string will answer well), drawn through the hive at the point of junction. The operator will next expel the bees from this box or story, by lifting the top-cover, and blowing in a little smoke, which will cause the inhabitants to retreat quickly to the lower regions. The box may then be taken away, without the operator running the risk of the slightest annoyance. The same effect may be produced by driving. The honey found in this removed box, will not be all honey of the current season, and consequently is not so delicately fine. It is also sometimes found mixed with, or rather deposited above, a layer of farina. Should it be wished, therefore, to obtain a supply free from these imperfections, the empty story which is added may be placed *above*, instead of *below* the original stock, and the honey will thus be of a superior kind. This mode of operating is called *super*-ing, in contra-distinction to *nadir*-ing.*

This practice of partial deprivation has never yet

* Dr Bevan practises Nadir-ing only with young swarms, and Super-ing with those of preceding years.

become general, because it is liable to frequent failure, even in improved hives, and because the full benefit is not derived from it at the very commencement of the system. The liability to failure, the first of the objections stated, is owing in most instances, not to the *mode*, but to the *period* of the operation. According to the too common practice of those who are friendly to deprivation, a portion of honey is abstracted from the hives about the beginning or middle of September; and the owner compliments himself on his moderation in being content with a part instead of the whole, and on his humanity in saving the lives of his industrious favourites; while in nine instances out of ten, he finds, on the arrival of March, that his moderation and humanity have been altogether unavailing; and that he has saved them from a violent death by suffocation, only to expose them to the more tardy, but not less cruel death, by starvation. Whereas, if deprivation take place soon after the swarming season, as already recommended, and is managed with discretion, the issue will be very different, and ultimately more profitable to the owner, than the almost universally practised mode by suffocation, which is too well known to need description. The latter system may yield a greater return in proportion to the hives operated upon,—but in the former there is a much greater number of hives available.

We are now to compare the *Suffocating System* with that by which, even though we defer the honey harvest to the usual late period of September, we may obtain the same quantity of produce, and at the same time save the lives of the bees. "Were we to kill the hen for her egg," says Wildman indignantly, "the cow for her milk, or the sheep for the fleece it bears, every one would instantly see how much we should act contrary to our interest; and yet this is practised every year in our inhuman and impolitic slaughter of the bees." It is mortifying to find writers of some celebrity in this branch of rural economy, defending the practice of suffocation, and using such arguments as the following:—"If he who dines every day on a good dish of animal food, does not find fault with the farmer who sold his cattle to the butcher, or who carried them to the market after he had himself cut their throats,—why does he exclaim against the Bee-cultivator who suffocates insects destined by Nature to die in the following year?"* Independent of the consideration that the carcase of the bee is not, like that of the sheep or ox, of use after its death, and that advantage may be derived from it while in life, the cold calculating spirit which could approve and recommend such uncalled-for barbarity, seems very inconsistent with the enthusiastic admiration of the insect generally felt by apiarians, and betrays more of the selfishness of the honey-merchant, than the generous feelings of the delighted Naturalist.

It is as clear as day, that the advantage of the owner is best consulted by saving the lives of his bees; because, independent of the satisfaction of eschewing the odious task of sacrificing what we have long watched with so much anxiety, and contemplated with so much admiration, the conservative system yields as large, if not a larger produce than the destructive, with this additional advantage, that the honey is not deteriorated by the unwholesome fumes of the sulphur made use of in suffocation; and, in the next place, we have the industrious collectors themselves ready in another season to renew their labours and add to our riches,—and requiring only to be united to some well-provisioned stock-hive which can afford to maintain them. It is pitiable to reflect that the small degree of additional trouble required in uniting them,† should

* Feburier, Traité des Abeilles.

† The French call this operation "marrying hives."

prove so effectual an obstacle to this conservative practice. Yet the operation with each hive so treated, need not occupy more than fifteen or twenty minutes. In the evening when all are quiet, turn up the hive which is to be operated upon, fixing it in a chair from which the stuffed bottom has been removed ; place an empty hive above it, wrap a cloth round the point of junction, to prevent the bees from coming out and annoying the operator ; then, with a short stick or stone in each hand, beat round the sides but *gently* for fear of loosening the combs. In five minutes the panic-struck insects will hastily mount into the empty hive, with a loud humming noise expressive of their trepidation. The hives are then separated,—that containing the bees is placed on its usual pedestal,—and the other containing the honey is carried off. The union is next to be effected. Turn up the stock-hive which is to receive the addition to its population,—with a bunch of feathers, or a small watering-pan, such as is used for watering flower-beds, drench them with a solution of ale and sugar, or water and sugar, made a little warm. Do the same to the expelled bees ; and then placing these last over the stock, mouth to mouth, a smart rap on the top of the hive will drive them down among the bees and combs of the undermost hive. Place this last on its pedestal, and the operation is completed. The strong flavour of the solution will prevent them from distinguishing between friend and stranger ; and their first movement, after recovering from their panic, will be to lick the liquid from one another's bodies. This mode of operating is applicable to all kinds of hives. It will be an advantage, though attended with a little additional trouble, to search for, and destroy the queen of the expelled bees, before the union takes place. Two queens cannot subsist together in one hive.—*Ibid.*

XIV.—*The Union of Swarms with their Stocks, and* *of Swarms or Stocks with each other*, in case of their being or becoming weak, has been attempted in various ways, and with various success, depending perhaps, in some degree, upon the skill and adroitness of the operator. Upon the storifying plan this operation will rarely be necessary, excepting in the case of weak stocks, as it is not a very common occurrence for storified bees to swarm, and when they do so, they generally throw off strong swarms. Still the object may occasionally be desirable, and it is worthy of attention, for *the tenants of well-filled hives are always the most active.*

The three usual methods by which union has been attempted, and indeed, their advocates say, accomplished, are *fuming them,—immersing them in water,*—and *aspersing them with sugared or honeyed ale.* To these we may add a fourth, namely, *operating upon their fears,* by confining them for a time, and then alarming them by drumming smartly upon the outside of their domicile. It was operating on their fears that enabled Wildman to perform such extraordinary feats with bees. When under a strong impression of fear, says he, they are rendered subservient to our wills to such a degree as to remain long attached to any place they afterwards settle upon, and will become so mild and tractable, as to bear any handling which does not hurt them, without the least show of resentment.

The neatest and most scientific mode with which we are acquainted *of uniting weak families together in harmony* was invented by the Reverend Richard Walond, whose experience in the management of bees, for nearly half a century, entitle his opinions concerning them to great respect. His theory and practice upon this subject are as follow:—'Bees,' says he, 'emit a peculiar odour, and it is by no means improbable that every family of bees emits an odour peculiar to itself; if so, as their vision seems to be imperfect, and their smell acute, it may be by this distinctive and peculiar odour that they are enabled to discriminate betwixt the individuals of their own family and those of a stranger hive. Upon this supposition, if the odours of two separate stocks or swarms can be so blended as to make them completely merge into each other, there will then probably be no difficulty in effecting the union of any two families that it may be desirable to unite. To accomplish this end, therefore, Mr Walond had recourse to a very ingenious contrivance: he procured a plate of tin, the size of a divider, and thickly perforated with holes, about the size of those in a course nutmeg-grater. Having confined, in their respective hives or boxes, the two families to be united, and placed over each other, with only a divider between them, he introduced his perforated tin plate upon the divider, which was then withdrawn. Immediately the bees began to cluster with hostile intentions, one family clinging to the upper, the other to the under side of the perforated plate; when, after remaining in this state for about twenty-four hours, they had so far communicated to each other their respective effluvia, and so completely commixed were the odours in both hives, that on withdrawing the perforated plate, the bees mingled together as one family: no disturbance was excited, but such as arose from the presence of two queens, the custom being always, in such case, to dethrone one of them. According to Huber this is effected by single combat between the queens. Keys has observed that *these incorporations seldom turn to account unless they be effected in summer;* and when it is considered that the principal gathering months are May and June, (excepting in those neighbourhoods that abound in lime, sycamore, and other trees that are apt to be affected with honey-dew,) we cannot, of course, expect them to be very successful.

This plan of the Rev. Richard Walond is very ingenious, and unquestionably, on his authority, proves our position,—that smell is one of the senses used by the bees to detect a stranger—and lead us to doubt the authenticity of accounts which state that the system of uniting casts by means of driving has been uniformly successful.

Our aim, however, is not to condemn, but to shew that fumigation is the easiest and surest operation.

The plan is as follows:—In the autumn, three or four fuzz-balls, or puff-balls (a kind of fungus growing in the meadows, and commonly called "the Devil's Snuff-box"), must be pulled before they are fully ripe. These must be thoroughly dried in an oven, and kept dry till wanted. A round box, made of thick tin, without any solder, must be provided. This box must be about two inches in diameter, and an inch and a half deep, with a conical moveable top, about an inch and a half high, perforated with holes. The bottom must also have three holes in it. With this box, and a piece of a fuzz-ball, about the size of a hen's egg, in readiness, the operator commences by fixing an empty hive, of the same size as that from which he intends to take the bees, securely, in an inverted position, in a pail, or some other convenient utensil. A sharp pointed stick having been stuck into the empty hive, so as to stand upright within it, the box is fixed thereupon, by inserting the stick into one of the holes in its bottom. The piece of fuzz-ball is then lighted and put in the box, over which the conical lid is placed. The hive from which the bees are to be taken is then placed over the empty hive and the burning fungus. To keep all close, a wet cloth is put round the place where the two hives join. In a minute or two the bees may be heard dropping heavily into the empty hive, where they lie stupified. After a short lapse of time, the full hive may be tapped, to cause the bees to fall faster. On removing the upper hive, the bees from it will all be found lying quiet at the bottom of the lower one. The queen may be taken from them and placed under a glass, with a little honey on a small piece of comb. The stupified bees must then be sprinkled freely with a thick syrup made of sugar and ale boiled together. The hive containing the bees with which it is intended to unite the stupified bees must now be placed on the top of that containing the latter, just as the hive was from which they have dropped. A cloth must be closely fastened round the two hives, so as to prevent any of the bees from escaping. The hives in this position must be put aside, where they will not be likely to be thrown down or disturbed. The bees in the upper hive, attracted by the scent of the syrup, go down and begin to lick the sprinkled bees clean. The latter gradually revive, and all get mingled together, and ascend quietly in company to the upper hive, where they dwell as if they had always been one family. The two hives should be left undisturbed for twenty-four or thirty hours, at the end of which the upper hive is to be removed and placed immediately on the spot from whence it was taken. The object of taking the queen away is to avoid all risk of disagreement. It is, however, recommended to preserve her as long as she will live, lest any accident should happen to the sovereign of the other community.—*Partly from Bagster.*

ANALYTICAL INDEX.

APPENDIX.

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